Prospective Memory and Intention Deactivation: Challenges, Mechanisms and Modulators

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Dipl.-Psych. Marcus Möschl

Reviewer: Prof. Dr. Thomas Goschke (TU Dresden, Germany)
Prof. Dr. Jan Rummel (Heidelberg University, Germany)

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M. Möschl and M. Walser developed the research question and conceptualized the review. M. Möschl conducted the project and wrote the manuscript under supervision of M. Walser and R. Fischer, who together with the other authors provided critical revisions.
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Summary

From the simple act of picking up a glass of water while talking to someone at a party, to remembering to swing by the bike shop to pick up an inner tube while riding through traffic on our way home from the office, intentions guide and alter our behavior—often while we are busily engaged in other ongoing tasks. Particularly, performing delayed intentions, like stopping at the bike shop on our way home, relies on a set of cognitive processes summarized as prospective memory (PM) that enable us to postpone intended actions until a later point in time (time-based PM) or until specific reminders or PM cues signal the appropriate opportunity to retrieve and perform an intended action (event-based PM). Interestingly, over the past decades a growing number of studies showed that successfully completing an event-based intention does not necessarily lead to its immediate deactivation. Instead, no-longer-relevant PM cues can incur so-called aftereffects that impair task performance and sometimes even trigger erroneous repetitions of the intended action (i.e., commission errors). Although in our everyday lifes we frequently rely on both PM and intention deactivation, still relatively little is known about how our cognitive system actually manages to deactivate completed intentions, under which conditions this may fail, and how well PM and intention deactivation function under extreme conditions, like acute stress.

In order to answer these questions, I first conducted a comprehensive review of the published literature on aftereffects of completed intentions. Here, I found that although intentions can incur aftereffects in terms of commission errors and performance costs that most likely result from continued intention retrieval, they generally seem to be deactivated or even inhibited at some point. Most importantly, this deactivation process does not operate like a light switch but dynamically moves along a continuum from complete reactivation to complete deactivation of intentions, and is substantially modulated by factors that also affect retrieval of intentions prior to their completion. Specifically, intention deactivation is most likely to fail when we remain within the same context in which we originally completed the intention and encounter no-longer-relevant PM cues that are extremely salient and were strongly linked to the intended action.

Subsequently, in Study 1 I directly tested a dual-mechanisms account of aftereffects of completed intentions. Building on findings of impaired intention deactivation in older adults who often show deficits in cognitive-control abilities, this account posits that

aftereffects and commission errors in particular stem from a failure to exert cognitive control when no-longer-relevant PM cues trigger retrieval of an intention. Accordingly, intention deactivation should hinge on the availability of cognitive-control resources at the moment we encounter no-longer-relevant PM cues. In order to test this, I assessed aftereffects of completed intentions in younger and older adults while manipulating transient demands on information processing during encounters of no-longer-relevant PM cues on a trial-by-trial basis. In Experiment 1, nominally more older adults than younger adults made a commission error. Additionally, medium demands on cognitive control substantially reduced aftereffects compared to low and high demands (i.e., u-shaped relation). In Experiment 2, which extended this manipulation but only tested younger adults, however, this control-demand effect did not replicate. Instead, aftereffects occurred regardless of cognitive-control demands. The lack of a consistent control-demand effect on aftereffects across two experiments, suggested that cognitive control either only plays a minor role for the occurrence of aftereffects or that, more likely, intention deactivation hinges on other specific cognitive-control abilities, like response inhibition.

In two subsequent studies, I extended this research and tested the effects of acute stress—a potent modulator of cognitive-control functioning—on PM and intention deactivation. Previous studies showed that, under moderate demands, acute stress had no effect on PM-cue detection, intention deactivation or performance costs that presumably arise from monitoring for PM cues. Importantly, however, based on these studies it remained unclear if acute stress affects PM and intention deactivation under high demands, as has been observed, for instance, with working-memory performance. To test such a potential demand-dependence of acute stress effects on PM, I first assessed the effects of psychosocial stress induction with the Trier Social Stress Test on PM and intention deactivation when detecting PM cues and intention deactivation were either low or high demanding (Study 2). Building on this work, I then tested the effects of combined physiological and psychosocial stress induction with the Maastricht Acute Stress Test on PM and the ability to track one's own performance (i.e., output monitoring), when PM-cue detection was difficult and ongoing tasks additionally posed either low or high demands on working memory (Study 3). Despite successful stress induction (e.g., increased levels of salivary cortisol and impaired subjective mood), both studies showed that PM-cue detection and intention retrieval were not affected by acute stress under any of these conditions. Study 2 revealed a tendency for a higher risk of making commission errors under stress

when no-longer-relevant PM cues were salient and difficult to ignore. Study 3 additionally showed that acute stress had no effect on output monitoring. Most importantly, however, across the different PM tasks and stress-induction protocols in these studies, acute stress substantially reduced performance costs from monitoring for PM cues, but did so only when PM-cue detection was difficult. This effect suggested that, depending on task demands, acute stress might shift retrieval processes in PM away from costly monitoring-based retrieval towards a more economic spontaneous retrieval of intended actions.

In summary, the present thesis suggests that the processes underlying prospective remembering and intention deactivation are tightly woven together and are only selectively affected by cognitive-control availability and effects of acute stress. With this, it contributed substantially to our understanding of these essential cognitive capacities and their reliability. My research showed that PM is remarkably resilient against effects of acute stress experiences when remembering intended actions is supported by external reminders. Acute stress may actually make monitoring for such reminders more efficient when they are hard to detect. Additionally, it showed that, in most circumstances, we seem to be able to successfully and quickly deactivate intentions once they are completed. It is only under some conditions that intention deactivation may be slow, sporadic or fail, which can lead to continued retrieval of completed intentions. While this seems not to be affected by transient demands on information processing during encounters of no-longer-relevant PM cues, intention deactivation might become difficult for older adults and stressed individuals when no-longer-relevant reminders of intentions easily trigger the associated action and are hard to ignore.

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1. Introduction

Like Odysseus, who continued his journey to Ithaca despite facing many perils and temptations that often required costly detours on his way (e.g., Homer & Stein, 2008), in everyday life, we often pursue goals in the face of distractions but are also able to change plans and adapt to environmental changes when goal pursuit becomes irrelevant. The cognitive capacity that enables such goal-directed yet flexible behavior is often referred to as volition or cognitive control (Botvinick, Braver, Barch, Carter, & Cohen, 2001; Kuhl, 1996). Its building blocks are intentions: they guide and shape our attention, thoughts and actions (Bratman, 1987; Goschke & Bolte, 2018; Lewin, 1926; Meiran, Cole, & Braver, 2012). In many cases, intentions alter our immediate actions. For instance, when we grasp a glass of water to have a drink, our intention to do so prompted us to align our gaze towards where we want to grasp the glass and align our hand orientation and movement in a way that— at least in most cases—enables us to have a drink without pouring water over ourselves (Herbort, Kirsch, & Kunde, 2019; Rosenbaum, 2009; for a brief introduction, see Rosenbaum, Herbort, van der Wei, & Weiss, 2014). In other cases, intentions are more future-directed. They cannot or should not be performed immediately, but need to be postponed and remembered until later, while we pursue different ongoing activities or tasks. For instance, when we intend to call a friend after a meeting at the office, we need to postpone and maintain that intention until after the meeting and then remember to make the phone call. Such prospective intentions may not necessarily change our immediate actions, but affect us in other ways. We might recognize intention-related information more quickly (Badets, Blandin, Bouquet, & Shea, 2006; Goschke & Kuhl, 1993) or might experience costs in ongoing tasks that we perform while maintaining and pursuing an intention (Heathcote, Loft, & Remington, 2015; McDaniel & Einstein, 2000; Meier & Rey-Mermet, 2018). Importantly, over the last two decades, a growing body of research suggested that intentions are not necessarily deactivated as soon as they have been completed or become irrelevant in other ways. Rather, they can continue to affect behavior through aftereffects, which are often detrimental to task performance and may even incur erroneous repetitions of intended actions (i.e., commission errors) (e.g., Marsh, Hicks, & Bink, 1998; Scullin, Bugg, & McDaniel, 2012; Walser, Fischer, & Goschke, 2012). These two cognitive functions—remembering and deactivating intentions—and particularly their underlying mechanisms and modulators are the main subjects of my dissertation.

The cognitive processes that enable us to postpone, remember and retrieve intentions have been investigated extensively under the term prospective memory (PM) (for overviews, see Brandimonte, Einstein, & McDaniel, 2014; A.-L. Cohen & Hicks, 2017a; Kliegel, McDaniel, & Einstein, 2008; McDaniel & Einstein, 2007). Diary- and questionnaire studies of PM have estimated that it is involved in up to 50–80% of everyday memory failures (Crovitz & Daniel, 1984; Kliegel & Martin, 2003; Terry, 1988) and recent advances in experience sampling suggest that throughout a day we think about postponed intentions approx. 13–15% of the time (Anderson & McDaniel, 2019; Gardner & Ascoli, 2015). Additionally, reports of participants with damage to the prefrontal cortex frequently failing to complete intended actions (e.g., Shallice & Burgess, 1991) or being are unable to resume tasks after interruptions (Uretzky & Gilboa, 2010), further illustrate the importance of PM for our day-to-day lifes.

Since its rekindling in the late-1980s (Kvavilashvili, 1987), research on PM has made enormous progress identifying underlying processes and modulating factors of successful and unsuccessful prospective remembering (e.g., A.-L. Cohen & Hicks, 2017a; Kliegel, McDaniel, et al., 2008; McDaniel, Umanath, Einstein, & Waldum, 2015). Moreover, since then, research on intention deactivation has found that although intentions often seem to be deactivated or even inhibited rather quickly after completion (Badets et al., 2006; Förster, Liberman, & Higgins, 2005; Marsh et al., 1998), this intention deactivation may fail and intentions can continue to affect behavior, as indicated by aftereffects even after completion (Anderson & Einstein, 2017; Scullin et al., 2012; Walser et al., 2012)—a finding that has recently been linked to deficits in cognitive-control functions, particularly in older adults (e.g., Bugg & Scullin, 2013). Interestingly, despite the relevance of both PM and intention deactivation in everyday life, little is known about how (well) these capacities and their sub-processes function in highly demanding conditions—for instance, under acute stress. While stress research has brought about a wealth of studies that investigated acute stress effects on a number of higher-order cognitive functions (for an overview, see Shields, Sazma, & Yonelinas, 2016), at the beginning of my dissertation, only a handful had assessed its effects on PM (Nater et al., 2006; Piefke & Glienke, 2017; Walser, Fischer, Goschke, Kirschbaum, & Plessow, 2013).

Against the backdrop of this research, the main aim of my dissertation was answering the following questions: What are potential mechanisms trough which our cognitive system manages to deactivate completed intentions? Under which circumstances can intention deactivation fail? Which role does the availability of cognitive-control resources play for the occurrence of aftereffects of completed intentions? In what ways and under which conditions does acute stress affect prospective remembering, its associated processes and intention deactivation?

In the following sections I will first give a brief introduction into the cognitive processes involved in PM, the paradigms used to assess them and the currently dominant theoretical view of intention retrieval. Subsequently, I will sketch out the general idea and procedures to assess intention deactivation, point out a central disparity between findings from different research lines on aftereffects of completed intentions, and introduce a recently developed theory about the nature of aftereffects of completed intentions that both motivated the first part of my dissertation. I will then introduce two important physiological changes following experiences of acute stress and their neurological effects, discuss why these could affect prospective remembering and intention deactivation and point out why it would be too early to conclude that acute stress does not affect PM, even though the extant studies suggested this. Finally, in the last part of this introduction, I will lay out the series of experiments and theoretical analyses I conducted during my dissertation in order to answer the questions I posed here.

1.1. Prospective Memory

Remembering and postponing intentions for later performance is a multi-phasic process that entails remembering several elements of an intention and involves multiple processes that enable actual retrieval of an intended action. It requires remembering that one wanted to do something (prospective component or memory for intent), what one intended to do and when this should be performed (the semantic content or retrospective component of PM) (e.g., Ellis & Freeman, 2008). According to the phase-or process model of PM (e.g., Ellis, 1996; Kliegel, Martin, McDaniel, & Einstein, 2002) after forming an intention, these elements are maintained either actively in working memory or over longer delays presumably in long-term memory while engaging in (an) unrelated ongoing task(s). When the right circumstances are met to perform the intention, we either need to inhibit ongoing-task performance and switch directly to intention execution or delay intention execution until after ongoing-task performance, depending on the nature of the intention (e.g., Bisiacchi, Schiff, Ciccola, & Kliegel, 2009). This intention execution is then followed by an evaluation of whether the intended action was completed successfully, which for complex multi-step intentions includes evaluating which step was performed—sometimes referred to as output monitoring (e.g., A.-L. Cohen & Hicks, 2017b; Koriat, Ben-Zur, & Sheffer, 1988).

Historically, PM research started out using naturalistic tasks, like instructing participants to mail postcards to a researcher on a specific date (e.g., Meacham & Singer, 1977; but see, Kvavilashvili, 1987; Kliegel, McDaniel, et al., 2008). While to date some studies also simulate such real-life intentions (Craik & Bialystok, 2006; Glienke & Piefke, 2016; Rendell & Craik, 2000), the majority of PM research adopted a laboratory PM paradigm that was introduced by Einstein and McDaniel (1990). Mimicking real-life circumstances, in these paradigms participants typically engage in an attention-demanding ongoing task while they delay performing a specific action until a certain point in time is reached (time-based PM task) or a pre-specified PM cue signals the appropriate retrieval opportunity (event-based PM task; for additional classifications, see Dismukes, 2012). For instance, participants might be instructed to perform a rather simple intention (e.g., a button press) whenever a certain amount of time has passed (e.g., 5 min) or a rare PM cue (about 1–15% of trials) signals a retrieval opportunity (Figure 3). The central measure of interest in these tasks often is PM performance, that is, the percentage of correctly detected PM cues, which is sometimes supplemented with response times (RTs) for performing the intended action.

According to the influential multiprocess view of PM, retrieving postponed intentions in such PM paradigms is enabled by two distinct processes: A bottom-up triggered spontaneous intention retrieval and a more top-down controlled process of monitoring the environment for retrieval opportunities (McDaniel & Einstein, 2000). While spontaneous retrieval is thought to be almost automatic, PM monitoring is thought to be a resource-demanding process that incurs measurable performance costs due to resource sharing with ongoing-task processing (Anderson, Rummel, & McDaniel, 2018; Rummel, Smeekens, & Kane, 2017; McDaniel et al., 2015; cf. Heathcote et al., 2015; Strickland, Heathcote, Remington, & Loft, 2017). Importantly, and in contrast to other views that suggest intention retrieval always relies on preparatory-attentional monitoring processes (PAM theory; e.g., R. E. Smith, 2010; R. E. Smith, Hunt, McVay, & McConnell, 2007) or the engagement of a resource-consuming retrieval mode and checking processes (e.g., Guynn, 2003), the multiprocess view proposes that PM monitoring and spontaneous retrieval are engaged dynamically. Specifically, the engagement of either process moves along a continuum from heavily monitoring-based intention retrieval to spontaneous intention retrieval without necessarily engaging any monitoring. Intention retrieval can be completely spontaneous when PM cues are rare and salient events with a high processing overlap with the ongoing-task at hand—that is, PM-cue detection requires the same processes needed to perform the ongoing task (focal PM cue; e.g., detecting the word *orange* in a lexical-decision task). Intention retrieval moves more towards engagement of monitoring processes when PM cues become more frequent and salience and processing overlap decreases—that is, PM-cue detection requires additional processing beyond the ongoing task (nonfocal PM cue; e.g., detecting words with the syllable *tor* in a lexical-decision task). Engagement of PM monitoring is further modulated by meta-control beliefs or expectations about PM-cue encounters, task demands or one's own performance (Ball, Brewer, Loft, & Bowden, 2014; Rummel, Kuhlmann, & Touron, 2013; Rummel et al., 2013; Scullin, McDaniel, & Shelton, 2013; Shelton & Scullin, 2017; R. E. Smith, Hunt, & Murray, 2017) and can also be adjusted flexibly according to context features of a task (A.-L. Cohen, Gordon, Jaudas, Hefer, & Dreisbach, 2017; Hefer, Cohen, Jaudas, & Dreisbach, 2017; Kuhlmann & Rummel, 2014; Lourenço & Maylor, 2014).

Beyond research on the specifics of intention retrieval processes and their neurological underpinnings (Burgess, Gonen-Yaacovi, & Volle, 2011; Cona, Scarpazza, Sartori, Moscovitch, & Bisiacchi, 2015; Gilbert, Hadjipavlou, & Raoelison, 2013; Horn, Bayen, Smith, & Boywitt, 2011), two topics have only rather recently gained interest in PM research: How does our cognitive system manage to deactivate intentions once they become irrelevant or are completed? How well do PM and intention deactivation function in extreme situations, like after acute stress experiences? In the following sections I will introduce the basic concepts and procedures to assess intention deactivation and describe a challenge this research currently faces. Then I will discuss the potential relevance of cognitive-control abilities for the occurrence of aftereffects, describe how acute stress could affect both PM and intention deactivation, summarize what we know about these issues so far and which open questions previous research on them poses.

1.2. Intention Deactivation

While PM research is concerned with the processes that enable and modulate retrieval of postponed intentions, research on intention deactivation is concerned with the questions whether and to what extent the processes triggered by intention-related stimuli or PM cues before or during active pursuit of intentions can be observed or change after an intention was completed successfully. Consequently, it often employs the same paradigms as research on PM. From a historic perspective, this research can be categorized into script-based studies that assess memory accessibility of complex action scripts (e.g., "setting a dinner table" or "clearing a desk"; Figure 1) and event-based PM studies that assess to what extent no-longer-relevant PM cues (i.e., $\mathrm{PM}_{\text{REPEATED}}$ cues) can trigger intention retrieval after completion (Figure 2). Rather than detailing the specifics of these research lines (see Chapter 2 for this), I will use the following section to illustrate a disparity between their findings that motivated the first part of research in my dissertation.

Early research by Zeigarnik (1938; see also Holton, 2009) and subsequent findings of a heightened memory accessibility of intention-related semantic concepts (i.e., intention-superiority effect; Goschke & Kuhl, 1993) suggested that the activation status of an intention might change with its completion. Building on this idea, Marsh et al. (1998; see also Marsh, Hicks, & Bryan, 1999) found that while intention-related memory contents were easily accessibility before intention completion, they seemed to actually become inhibited upon intention completion. That is, in a script-based paradigm, words from an action script that had recently been performed (e.g., *napkins* or *tablecloth*) incurred slower lexical decisions than words from an action script that had only been memorized but was never performed (e.g., *folder* or *pencil*). Although some follow-up studies also found that the heightened accessibility of uncompleted intention representations could persist residually after completion (A.-L. Cohen, Dixon, & Lindsay, 2005; Penningroth, 2011), other studies for the most part suggested that such cognitive intention representations (Badets et al., 2006; Meilán, 2008) and even semantic networks associated with a PM cue (Förster et al., 2005) are deactivated or even inhibited after intention completion.

In contrast to research in script-based paradigms, research in event-based PM paradigms rather consistently showed that intentions are not necessarily deactivated or inhibited after completion, but that they seem to continue to be retrieved. That is, $\mathrm{PM}_{\text{REPEATED}}$ cues that signaled a retrieval opportunity while an intention was actively pursued, produced substantial performance costs in terms of slowed ongoing-task responses and sometimes even lead to commission errors after successful completion of an intention (Pink & Dodson, 2013; Scullin et al., 2012; Walser et al., 2012; cf. Scullin et al., 2012; Scullin, Einstein, & McDaniel, 2009). However, even within this line of research, aftereffects were only reported under some but not all conditions.

Importantly, on the surface, the findings from script-based and event-based PM studies seem highly contradictory: How can intentions be inhibited in one type of task but continue to be retrieved in another task? What are the reasons for this disparity, and is it possible to reconcile these findings? Moreover, if completed intentions produce aftereffects, what are the mechanisms behind them and, why do aftereffects only occur sometimes but not all the time? When I started working on this topic, it seemed incredibly difficult to answer these questions. This field had produced a considerable number of studies that assessed intention deactivation in a variety of tasks and dependent measures and came up with several diverse suggestions for mechanisms underlying aftereffects of completed intentions. Importantly, however, while it had started to bring together some of its findings, there seemed to be no comprehensive explanation for their heterogeneity between and even within different research lines. In an attempt to alleviate this theoretically unsatisfying situation, in Chapter 2, I will provide a first systematic overview, integration and interim conclusion about the mechanisms and modulators of intention deactivation and aftereffects of completed intentions.

1.3. Cognitive Control and Intention Deactivation

In addition to the general question whether completed intentions are deactivated or not, research on intention deactivation in event-based PM paradigms recently posed a more specific question regarding the involvement of cognitive-control capabilities and resources in the genesis of aftereffects. A recently formulated dual-mechanisms account suggests that commission errors in particular result from a failure to exert cognitive control after retrieval of a completed intention (Bugg & Scullin, 2013; Bugg, Scullin, & Rauvola, 2016). The idea is that encountering a $\text{PM}_{\text{REPEATED}}$ cue triggers spontaneous retrieval of the no-longer relevant intended action, which then needs to be controlled or inhibited in order to give an ongoing-task response. According to the dual-mechanisms account, commission errors emerge when control over intention execution fails. So far, this account receives support from correlative findings of a higher commission error risk and sometimes more frequent commission errors in older adults who showed deficits in inhibitory control functions (Bugg et al., 2016; Scullin et al., 2012; Scullin, Bugg, McDaniel, & Einstein, 2011) or in participants who were fatigued after an extended period of experimental testing, which is thought to have temporarily impaired inhibitory control (Scullin & Bugg, 2013). Importantly, if this dual-mechanisms explanation of aftereffects was correct, then their occurrence and size should hinge on the availability of cognitive-control resources—most critically—during encounters of PM$_{\text{REPEATED}}$ cues that trigger intention retrieval. In Study 1 (Chapter 3) I will provide a first direct test of the involvement of cognitive-control resources in intention deactivation.

1.4. Acute Stress, PM and Intention Deactivation

Acute stress is a powerful influence on cognitive functioning in everyday life that has even been deemed to be "ubiquitous and universally pervasive" (Chrousos, 2009, p. 379). Given the strong involvement of PM in everyday memory (Kliegel & Martin, 2003), reports of rising stress levels in the work place (e.g., S. Cohen, Janicki-Deverts, & Miller, 2007; Lohmann-Haislah, 2012), and our own experiences, it is almost certain that we rely on PM and intention deactivation also under stress. Astonishingly, compared to the wealth of studies about stress effects on cognitive-control functions, to date relatively little is known about whether acute stress affects PM in any way.

The complex pattern of physiological and neuronal changes following experiences of acute stress has been detailed comprehensively by Joëls and Baram (2009). For the sake of brevity, here I will focus on two major physiological stress effects: The immediate fast-paced activation of the sympathetic nervous system that is associated with the release of noradrenaline, dopamine and other catecholamines; and the slightly delayed and slow-paced increase of hypothalamus-pituitary-adrenal axis activity that is associated with the release of cortisol and other glucocorticoids into the bloodstream, mediated by the release of neuropeptides like corticotropin-releasing hormone (Charmandari, Tsigos, & Chrousos, 2005; Chrousos, 2009; Dickerson & Kemeny, 2004; Joëls & Baram, 2009). Depending, for instance, on the type and duration of a stressor, these physiological responses decrease neural activity and connectivity in the prefrontal cortex (Arnsten, 2009, 2015; van Oort et al., 2017), alter neural connectivity and long-term potentiation in the hippocampus (Kruse, Tapia León, Stalder, Stark, & Klucken, 2018; Schwabe, Joëls, Roozendaal, Wolf, & Oitzl, 2012), and increase sensitivity of the visual cortex (Shackman, Maxwell, McMenamin, Greischar, & Davidson, 2011; van Marle, Hermans, Qin, & Fernández, 2009). These changes in turn alter and often impair working memory, declarative learning and selective attention (for recent meta-analyses, see Shields, Bonner, & Moons, 2015; Shields, Sazma, McCullough, & Yonelinas, 2017; Shields et al., 2016).

Prospective memory could be susceptible to acute stress effects not only because theoretical models assume that it relies on stress-sensitive working memory and selective attention (e.g., Cona et al., 2015; Kliegel et al., 2002), but also because it relies on stress-sensitive brain structures: This has been demonstrated compellingly by reports of prefrontal-cortex lesions that lead to impaired event-based (or activity-based) PM performance (Carlesimo, di Paola, Fadda, Caltagirone, & Costa, 2014; Neulinger, Oram, Tinson, O'Gorman, & Shum, 2016; Shallice & Burgess, 1991; Umeda, Kurosaki, Terasawa, Kato, & Miyahara, 2011; Uretzky & Gilboa, 2010; but see Volle et al., 2011; Kinch & McDonald, 2001; Cockburn, 1996 for preserved event-based but impaired time-based PM after brain injury). Similarly, it is feasible that acute stress affects intention deactivation, since it is associated with stress-sensitive anterior cingulate and rostrolateral prefrontal cortex functioning (Beck, Ruge, Walser, & Goschke, 2014) and involves memory for stimulus–response associations that is sensitive to acute stress effects (Guenzel, Wolf, & Schwabe, 2013; Schwabe & Wolf, 2013).

Surprisingly, when I started my dissertation, only three experimental studies had assessed the effects of an acute-stress experience on PM (Glienke & Piefke, 2016; Nater et al., 2006; Walser et al., 2013). Even more surprising were their findings: In contrast to my own experiences and despite the putative pathways for acute stress effects on PM that I outlined above, these studies reported either no effects or seemingly positive effects of acute stress on PM. Nater et al. (2006) found that acute stress had no effect on event-based PM performance, but increased participants' clock-checking frequency in a time-based PM task, which coincided with more accurate PM performance. Walser et al. (2013) showed that acute stress had neither an effect on event-based PM performance, nor on PM monitoring or intention deactivation. Lastly, in contrast to both studies, Glienke and Piefke (2016), who used a complex real-life simulation of PM, found that about 50+ min after stress induction, acute stress had no effect on time-based PM performance, but stressed participants consistently showed accurate event-based PM performance over the course of testing, while PM performance declined in non-stressed participants. In parts, the divergence between findings from these studies can be attributed to substantial differences between PM paradigms or the time-sensitivity of acute stress effects on cognition (e.g., Plessow, Fischer, Kirschbaum, & Goschke, 2011; see also Schwabe & Wolf, 2013, 2014) and/or stress effects at different stages of memory encoding and retrieval (e.g., Schwabe, Joëls, et al., 2012). Nevertheless, regarding stress effects in standard PM paradigms, it would be too early to conclude that stress does not affect event-based PM based on the studies by Nater et al. (2006) and Walser et al. (2013).

Instead, the occurrence of acute stress effects on PM might depend on task demands. This has been demonstrated, for instance, in stress research on working memory (Oei, Everaerd, Elzinga, van Well, & Bermond, 2006; Schoofs, Preuß, & Wolf, 2008; Schoofs, Wolf, & Smeets, 2009) and fits with the observation that stress so far has been shown to affect time-based PM that is presumably associated with high demands on self-initiated intention retrieval (Nater et al., 2006). Also consistent with this idea, acute stress affected event-based PM in a demanding complex real-life simulation that required concurrent maintenance and coordination of multiple different event- and time-based PM tasks (Glienke & Piefke, 2016).

Potentially relevant factors could be the demands on PM-cue detection, as they have been shown to reliably affect event-based PM performance and PM-monitoring costs (Einstein & McDaniel, 2005; McDaniel et al., 2015), as well as intention deactivation (e.g., Scullin et al., 2012): Typically, PM performance and aftereffects decrease and PM-monitoring costs increase with nonsalient and nonfocal PM cues that are difficult to detect and easily overlooked or ignored, like the syllable *tor* in a word-categorization task. Conversely, PM performance and aftereffects increase with salient and focal PM cues that are easy to detect and difficult to ignore, like the word *fish* presented against a bright red background during a word categorization task (e.g., Anderson & Einstein, 2017). Additionally, PM performance is affected by ongoing-task demands and has, for instance, been reported to be lower during 2-back than during 1-back working-memory tasks for younger but not for older adults (West & Bowry, 2005). Importantly, the event-based PM tasks in the Nater et al. (2006) and Walser et al. (2013) studies employed simple ongoing tasks (i.e., word categorizations) with single nonsalient focal PM cues (i.e., specific words) which presumably posed moderate demands on PM-cue detection, PM monitoring (e.g., McDaniel et al., 2015) and intention deactivation (e.g., Scullin et al., 2012).

Hence, based on the previous studies, it remains unclear, whether acute stress would affect PM and intention deactivation under higher demands on PM-cue detection or ongoing-task performance. Additionally, so far no study assessed acute stress effects on output monitoring in PM. This refers to the ability to remember one’s past responses, which becomes particularly relevant when postponed intentions contain multiple sequential steps and require keeping track of performed actions (e.g., A.-L. Cohen & Hicks, 2017b; Kliegel, Mackinlay, & Jäger, 2008). Therefore, I will test this assumption in different tasks and different stress-induction protocols in Study 2 (Chapter 4) and Study 3 (Chapter 5).

1.5. Research Agenda

In order to target the main aim and the specific questions of my dissertation outlined in the previous chapters, I conducted the following series of theoretical analyses and experiments.

First, in Chapter 2, I review the extant literature on aftereffects of completed intentions and discuss the validity and propensity of procedures to measure aftereffects, their findings and the underlying mechanisms they convey. With this, I aim to establish a clear picture of where research on intention deactivation and aftereffects of completed intentions currently stands and attempt a theoretical advance towards understanding the set of mechanisms and processes underlying intention deactivation and aftereffects of completed intentions in order to provide a unifying perspective on these phenomena.

Second, in Study 1 (Chapter 3), I investigated the recent suggestion that aftereffects result from failures of cognitive control after a completed intention has been retrieved spontaneously. This idea is based on several correlative findings of impaired intention deactivation in older adults who showed intact retrieval of active intentions but deficits in cognitive-control functions like response inhibition or selective attention (e.g., Bugg et al., 2016). Study 1 provides a direct test of this dual-mechanisms account: Here I assessed both older and younger adults' capacity for intention deactivation under varying degrees of available cognitive-control resources during encounters of no-longer-relevant PM cues—that is, when cognitive-control availability is presumably most critical for successful intention deactivation.

Third, in Study 2 and Study 3 (Chapters 4 & 5), I assessed the modulation of PM and intention deactivation by experiences of acute stress and the conditions under which such a modulation may occur. To this end, in Study 2, I tested the effects of acute psychosocial stress on several sub-processes of PM under varying demands on PM-cue detection, PM-cue maintenance (prospective load) and PM monitoring as well as under high demands on intention deactivation. Study 2 suggested that acute stress effects on PM depend upon specific task demands. Therefore, in Study 3, I extended this research and tested the effects of acute physiological and psychosocial stress on PM and output monitoring—the ability to keep track of and monitor one's own performance—under varying ongoing-task demands.

2. Aftereffects and Deactivation of Completed Prospective Memory Intentions: A Review

2.1. Abstract

Prospective memory, the ability to perform an intended action in the future, is an essential aspect of goal-directed behavior. Intentions guide our behavior and shape the way we process and interact with our environment. One important question for research on prospective memory and goal-directed behavior is whether this influence stops after the intention has been successfully completed. That is, are intention representations deactivated from memory after their completion, and if so, how? Here we review twenty years of research on intention deactivation and so-called aftereffects of completed intentions across different research fields. First, we present different paradigms assessing aftereffects of completed intentions and their corresponding findings as to whether completed intentions are inhibited, directly deactivated, continue to be retrieved or remain residually activated. While early studies that assessed memory accessibility of intention-related semantic networks proposed a deactivation or even inhibition of completed intentions, more recent event-based prospective memory studies that assessed task interference from no-more-relevant prospective memory cues, mostly proposed continued retrieval or residual activation of completed intentions. Second, we discuss potential mechanisms underlying aftereffects of completed intentions as well as intention deactivation. Lastly, we propose some new directions and novel experimental procedures for future research on mechanisms and modulators of intention deactivation.

2.2. Statement of public significance

This review shows that intended actions can persistently affect performance even after they have been completed and become no longer relevant. Importantly, the deactivation of completed intentions does not work on an all-or-nothing basis but instead operates on a continuum from full intention retrieval to a complete deactivation or even inhibition, depending on a variety of factors.

2.3. Introduction

Adaptive goal-directed behavior not only requires adjusting to stimuli and needs of current situations but also requires disengaging from goals or experiences that become irrelevant. Disengagement failures or impairments can otherwise drastically impair subsequent performance and hinder pursuit of novel goals (Allport, Styles, & Hsieh, 1994; Mayr & Keele, 2000). This has been demonstrated in terms of impaired performance due to ruminations about failures (Beckmann, 1994) or previous tasks (in individuals with major depression; Whitmer & Gotlib, 2012), and, more prominently, in terms of massive impairments in everyday life due to intrusions of traumatic events into memory (in individuals with posttraumatic stress disorder; Ehlers & Clark, 2000). Similarly, failing to deactivate completed and thus no-longer relevant prospective memory (PM) intentions can incur so-called aftereffects that impair focus on subsequent tasks and, more importantly, might also lead to performance of the intention when it is no longer relevant such as accidental overmedication (Gray, Mahoney, & Blough, 2001; Kimmel et al., 2007). At least in part, such deactivation failures might also account for perseverative behaviors as have been observed after prefrontal-cortex lesions (Milner, 1963; Owen, Roberts, Hodges, & Robbins, 1993) or in individuals with obsessive-compulsive disorder (e.g., Menzies et al., 2008).

The aim of the present chapter is to review the rapidly expanding literature on intention deactivation in order to provide a much-needed progress report on studies that investigated aftereffects of completed intentions and the mechanisms behind them. To this aim, we will first detail basic paradigms that were used to assess aftereffects as well as their corresponding findings. Second, we will discuss potential mechanisms of aftereffects and potential underlying mechanisms of successful and unsuccessful intention deactivation. Finally, we will conclude by proposing an integrative perspective on aftereffects findings and identify future avenues within this research area.

2.3.1. Study Selection and Inclusion Criteria

For this review we conducted a search of the databases PubMED, PsycARTICLES, PsycINFO and Web of Science for articles published until November 5, 2018, with the following search string: (("prospective memory" OR "intention*") AND ("completed intention*" OR "intention completion" OR "finished intention*" OR "fulfilled intention*" OR "intention fulfillment" OR "completed goal*" OR "goal completion" OR "finished goal*" OR "fulfilled goal*" OR "goal fulfillment" OR "completed task*" OR "task completion" OR "finished task*" OR "fulfilled task*" OR "task fulfillment" OR "intention interference" OR "intention deactivation" OR "intention inhibition" OR "goal inhibition" OR "goal deactivation" OR "persisting activation" OR "residual activation" OR "commission error*" OR "aftereffect*" OR "negative prospective memory" OR "deactivation process*")). This search resulted in 33 findings in PubMED (using the qualifier [Title/Abstract] after each quoted search term), 18 in PsycARTICLES, 135 in PsycINFO (using the NOFT prefix before the entire search string), and 176 in Web of Science (using the "Topics" search).

We used the following inclusion criteria to select relevant articles: Peer-reviewed articles that used (a) laboratory paradigms, in which participants had to perform a prospective memory task and after intention completion were engaged in a finished PM phase in which activation of the completed intention representation was assessed; (b) naturalistic studies in which recognition or recall of performed versus to-be-performed intentions were assessed. Additionally, we examined references from relevant papers to detect potentially relevant articles that were not retrieved by the databases. Further, we set Google scholar alerts until November 20, 2018, to detect new articles citing our pre-selected articles. This search strategy led to the inclusion of 34 studies.

2.4. Paradigms to Assess Aftereffects of Completed Intentions

Since early days of research on voluntary action and “the will”, philosophers and psychologists assumed that intentions or goals are characterized by *special* properties. For instance, researchers argued that intentions are stable and controlling (Bratman, 1987; see also Holton, 2009) or that they induce a tension system or *quasi-need* for pursuing a goal (Lewin, 1926). Building on these ideas and early findings of higher recall rates for interrupted than for completed tasks (Zeigarnik, 1938) that supported the notion of intentions as special, Goschke and Kuhl (1993) initiated laboratory-based investigations of memory activation of uncompleted intentions. In their postponed intention paradigm, participants memorized two scripts of short action phrases before being informed which script they had to perform later (i.e., prospective script) and which script did not need to be performed (i.e., neutral script). Before script performance, participants recognized words of the to-be-performed prospective script faster than words of the neutral script. The authors concluded that intentions are stored in a heightened sub-threshold activation; an effect they referred to as the *intention-superiority effect* (for replications see A.-L. Cohen et al., 2005; A.-L. Cohen, Kantner, Dixon, & Lindsay, 2011; Marsh et al., 1998; Maylor, Darby, & Sala, 2000; Meilán, 2008; Schult & Steffens, 2017). This finding subsequently raised questions about what happens to the activation level of intentions once they are completed: Would intention representations be deactivated or continue to remain active?

To date, a number of different experimental procedures have been developed that use a variety of dependent measures to investigate aftereffects of completed intentions and the mechanisms supporting intention deactivation[1]. Some studies investigated aftereffects of completed intentions in terms of altered performance during encounters of intention-related stimuli. These studies were either script based (e.g., Marsh et al., 1998) or they used event-based PM tasks (e.g., Förster et al., 2005) and applied the same accessibility logic as in the postponed intention paradigm. However, in most studies experimenters have employed event-based PM paradigms that repeatedly presented the exact cue that was previously used to signal an opportunity to perform the intended action (e.g., Walser, Fischer, & Goschke, 2012; Scullin, Bugg, McDaniel, & Einstein, 2011). We will refer to such cues throughout this paper as $\mathrm{PM}_{\mathrm{REPEATED}}$ cues. Some studies of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues focus on response times (RTs), whereas others assess erroneous repetitions of intended actions—that is, commission errors—in response to $\mathrm{PM}_{\mathrm{REPEATED}}$ cues (e.g., Bugg & Scullin, 2013; Pink & Dodson, 2013). Additionally, some researchers have assessed thoughts about the intention after encountering $\mathrm{PM}_{\mathrm{REPEATED}}$ cues (Anderson & Einstein, 2017).

One central difference between these procedures to assess aftereffects of completed intentions is the interpretation of their findings. Script-based paradigms usually present words that refer to an intended action or to the objects of the action (e.g., “Spread the table cloth”). Increased activation of a completed intention will thus show up either in facilitation or interference effects, depending on the nature of the ongoing task: In facilitation tasks like lexical-decision or recognition tasks, high activation of intention-related concepts will speed up lexical access or retrieval. By contrast, in Stroop-like paradigms, in which intention-related words serve as distractors, high activation will produce increased interference and thus increased RTs. Predictions are qualitatively different for event-based PM paradigms in which $\mathrm{PM}_{\mathrm{REPEATED}}$ cues are presented. Here, increased activation of a completed intention will increase the likelihood that the

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[1] Note that the term aftereffects is also used in the PM literature when referring to performance on trials directly following retrieval of the PM intention in response to a target during phases of the task when the PM response is supposed to be performed (Meier & Rey-Mermet, 2012).

completed intention and/or the no-longer-relevant PM response is retrieved. This in turn should interfere with the ongoing task response and therefore produce increased RTs or commission errors. In the following section we will introduce some prototypical examples of procedures to assess aftereffects and review the findings and data interpretations prevalent in these studies (for an overview of the reviewed studies see Table 1).

2.4.1. Script Paradigms

Early studies on aftereffects of completed intentions were based on the aforementioned postponed-intention paradigm (Goschke & Kuhl, 1993) to assess memory activation or accessibility of the content (i.e., the specific actions) of intention representations. For instance, Marsh et al. (1998) instructed participants to memorize two scripts (e.g., *setting a table*) comprising short action phrases (e.g., *spread the table cloth*, *distribute the cutlery*) before they were informed which script they had to perform later (prospective script) and which not (neutral script) (Figure 1). In an uncompleted-intention condition, participants performed a lexical decision task before they performed the prospective script in which they responded faster to prospective-script words (e.g., *spread*, *cutlery*) than to neutral-script words. Because RTs and accessibility in memory are assumed to be inversely related (Ratcliff & McKoon, 1978), Marsh et al. (1998) interpreted this finding as an intention-superiority effect—that is, heightened sub-threshold activation of intention-related memory contents. In a completed-intention condition, in which participants performed a lexical decision task after performing the prospective script, they responded slower to prospective-script words than to neutral-script words. This was interpreted as a decrease in accessibility of intention-related memory contents and thought to be the result of an inhibition of semantic networks related to a completed intention. The latter finding was replicated and extended to unrelated action phrases that were not part of an overarching script (e.g., *distribute the cutlery, sharpen the pencil*), suggesting that unrelated intentions are subject to the same activation dynamics as related intentions that are part of an overarching script, as well as for intentions that were canceled before they could be performed (Marsh et al., 1999). Moreover, Badets, Blandin, Bouquet and Shea (2006) found similar results when assessing the accessibility of learned motor sequences. Participants in their study learned to trace line patterns by pressing and holding keys that moved a cursor. Participants who were instructed that they would perform the learned motor sequences at about 10 minutes after learning, recognized the corresponding line patterns faster prior to performance and slower afterwards than participants who only performed a recognition test immediately after learning. Similar to Marsh et al. (1998), Badets et al. (2006) interpreted their results in terms of increased accessibility of intended actions prior to completion and reduced accessibility or inhibition after completion (see also Badets, Albinet, & Blandin, 2012; for an overview of research on delayed motor intentions see Badets & Osiurak, 2015).

While these findings suggest that after completion, the content of related or unrelated verbal or motor intentions becomes inhibited, the notion of a post-completion intention inhibition is—even within this line of research—not undisputed. That is, other variations of the postponed-intention paradigm found either no RT differences between prospective- and neutral-script words (Meilán, 2008) or faster responses to prospective-script words relative to neutral-script words after intention completion (Penningroth, 2011), suggesting either a direct deactivation or persisting activation of completed intentions, respectively (Table 1). In line with the latter finding of persisting activation, Cohen et al. (2005) found that words from a previously performed prospective script increased RTs in a color-naming Stroop task more than unrelated control words that were not memorized beforehand. This enhanced “intention interference effect” also suggests that completed intentions exhibited heightened activation compared to novel stimuli and thus provides evidence for a residual activation of completed intentions.

Lastly, studies that investigated the accessibility of everyday life intentions found that participants recalled activities that were completed less frequently than activities they wanted to complete over the course of a certain time period (e.g., one week) (Freeman & Ellis, 2003; Maylor, Chater, & Brown, 2001; Maylor et al., 2000). While the use of naturalistic intentions provides high external validity, it is difficult to infer the mechanisms underlying the observations in these studies: An increased recall of to-be-completed over completed tasks may result from a deactivation or (partial) inhibition of completed tasks, but also from a facilitation of to-be-completed tasks, or both. Nevertheless, these studies provide further support for the notion that the activation levels of intention-related concepts are subject to change depending on the completion status of an intention.

On the surface, script-based studies of intention deactivation appear to produce divergent results. Part of this is due to the use of different paradigms and the way researchers interpreted slowed versus speeded responses to intention-related stimuli in facilitation or interference tasks. However, taken together, these studies suggest that after completion the activation of the content of an intention often but not always goes back to or even below baseline activation. These findings seem to fit well with everyday experiences of being able to focus on ongoing tasks and future tasks—most of the time without being (overly) distracted by recollecting tasks that we have already performed. Nevertheless, as foreshadowed in the preceding paragraphs, and as elaborated in the sections below, this general deactivation or inhibition process may be slow, sporadic, or faulty, leading to observations of heightened intention activation even after completion.

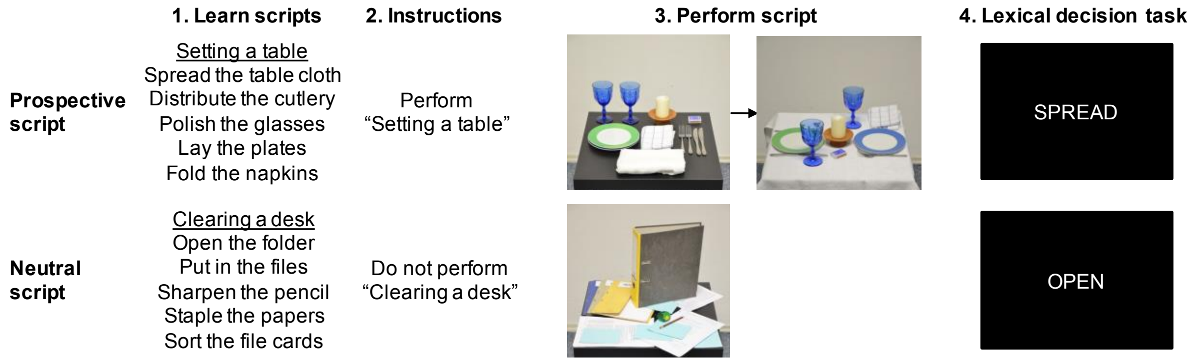

Figure 1. Procedure of a script paradigm adapted from the Marsh et al. (1998) study. A schematic illustration of the completed intention condition is shown. Participants first learned two scripts, each with five action phrases until they were able to reproduce them once completely in a written free recall test. Subsequently, participants were instructed which script they should perform later (i.e., prospective script) and which not (i.e., neutral script). Materials for script performance were set up at a table behind the computer. After prospective-script performance, aftereffects were assessed in a lexical decision task that contained words from the performed prospective script and the not-performed neutral script. In an uncompleted-intention condition, the lexical decision task preceded prospective-script performance.

2.4.2. Event-Based PM Paradigms

Apart from script paradigms, researchers often used event-based PM paradigms (Einstein & McDaniel, 1990) to assess aftereffects of completed intentions. In typical event-based PM paradigms participants are instructed to perform a simple intended action (e.g., pressing a specific key, such as F1) upon encountering specific, rarely occurring events (i.e., PM cues such as a specific word, picture, or symbol). Mirroring the real-world circumstances of being busily engaged in ongoing activities (e.g., driving a car) when needing to remember to complete an intention (e.g., return a phone call), in laboratory settings the PM task is embedded within an ongoing task (e.g., lexical decision, image rating). One key difference between event-based PM paradigms and script-based paradigms is that intended actions in event-based PM tasks are designed to be simple so that the prospective component of PM (needing to remember to do something) can be studied without minimal conflation with the retrospective component (what needs to be remembered; e.g., Ellis, Kvavilashvili, & Milne, 1999).

In one early line of research, Förster, Liberman and Higgins (2005) combined the accessibility logic of script paradigms (Goschke & Kuhl, 1993; Marsh et al., 1998) and event-based PM tasks to assess the accessibility of memory contents that were semantically related to PM cues from either uncompleted or completed PM tasks. In their study, participants performed four blocks of an image-rating task, each one followed by a lexical decision task. The PM task instructed participants to inform the experimenter whenever they saw a picture of glasses that was followed by a picture of scissors during the image-rating tasks. This PM-target combination was presented only once (during the third image-rating block). The lexical decision tasks in between the image ratings served to assess the accessibility of intention-related words. Before intention completion, that is, before the PM-target combination appeared, participants made faster lexical-decisions on words that were semantically related to the PM cue *glasses* (e.g., the words *professor*, *read*, and *sun*) than on unrelated control words. Applying the accessibility logic of script paradigms, the authors interpreted this finding as an intention-superiority effect. Most importantly, the relative advantage of PM-cue-related over unrelated words in the lexical-decision tasks was reduced after compared to before intention completion. Moreover, this effect was larger for participants in an intention group who were instructed to perform the PM task than for participants in a no-intention group who were not instructed to perform the PM task. Förster and colleagues (2005) also observed similar results, when substituting the lexical decision task for a Stroop color-naming task. That is, response slowing on PM-cue-related words compared to control words decreased after intention completion in an intention relative to a no-intention group. In line with previous script studies (Marsh et al., 1998, 1999), Förster et al. (2005) interpreted these findings as evidence for the inhibition of completed intention representations (see also Denzler, Förster, & Liberman, 2009; Denzler, Häfner, & Förster, 2011).

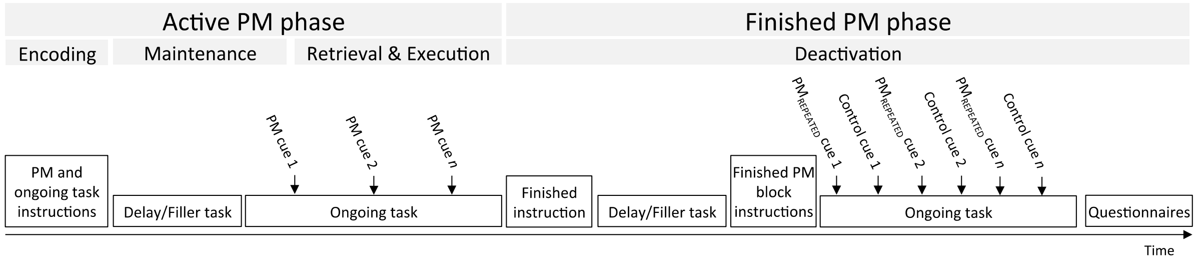

Figure 2. Main components of a laboratory event-based prospective memory (PM) paradigm to assess aftereffects of completed intentions.

In a second, more recent, line of research, several authors adopted event-based PM paradigms to assess accessibility or retrieval of intention representations, PM-cue–action links or intended actions by testing performance in trials that present $PM_{REPEATED}$ cues (Figure 2, Table 1). This shifts the focus of the paradigm away from assessing accessibility of intention-related memory contents towards assessing accessibility or retrieval of PM-cue representations and episodic bindings (stimulus–response links) between PM cues and intended actions. As in a standard event-based PM task (e.g., Einstein & McDaniel, 1990), in these paradigms participants were instructed to perform an ongoing task (e.g., lexical decisions or digit categorization) and an additional PM task that requires performing a specific action (e.g., press the “Q” key) whenever a pre-specified event (i.e., PM cue; e.g., the words corn or dancer, or a specific symbol such as a square) occurs within the ongoing task (Scullin et al., 2009; Walser, Goschke, & Fischer, 2014). After this “active PM phase” was finished, participants were explicitly instructed that the PM task was completed. In a subsequent “finished PM phase”[2], participants then performed another ongoing-task block, during which $PM_{REPEATED}$ cues appeared.

The specific measures of aftereffects depend on the implemented study design. In one type of study design, aftereffects were measured primarily in terms of commission errors that are defined as erroneous PM responses in $PM_{REPEATED}$ trials occurring up to two (Scullin & Bugg, 2013; Scullin et al., 2012) or three subsequent trials (Anderson & Einstein, 2017). Based on the reasoning that participants might change the way they approached the task after making a single commission error (e.g., participants might reconceptualize the finished PM phase as an active PM phase), these studies typically examine whether

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[2] Note that this phase of the experimental procedure has been referred to in many different ways such as “test block” (Walser, Fischer, & Goschke, 2012), “phase 2” (Pink & Dodson, 2013; Scullin, Bugg, & McDaniel, 2012), “completed phase” (Anderson & Einstein, 2017), “finished PM phase” (Bugg, Scullin, & Rauvola, 2016) and “block 2” (A.-L. Cohen, Gordon, Jaudas, Hefer, & Dreisbach, 2017). Here we decided to use an active/finished terminology to more clearly delineate phases of the experiment in which an PM task is “active” and should be performed compared to phases in which the PM task is “finished” and should not be performed.

experimental conditions affected the *risk* for making a commission error. That is, the number of participants that made at least one commission error was compared to the number that did not make a commission error (e.g., Scullin et al., 2012). Early studies suggested that commission errors were rare/absent when the PM cue was nonsalient and when the ongoing task differed across active and finished PM phases (Scullin et al., 2011). In subsequent studies, to avoid floor effects when measuring commission errors, the PM/PM$_{\text{REPEATED}}$ cues were presented in a salient background color (red or navy) and during the same ongoing task type across active and finished PM phases. Based on the multiprocess theory of prospective memory, these conditions were expected to promote spontaneous retrieval of the completed intention (e.g., McDaniel et al., 2015). Note that in this design participants usually performed a single active PM phase followed by a single finished PM phase.

A second type of study design extended the first approach and assessed ongoing-task RTs, error rates and/or commission-error rates in PM$_{\text{REPEATED}}$ trials compared to performance in unrelated control trials[3] as their main measures of aftereffects (e.g., Walser et al., 2012; see also Scullin et al., 2009) (Figure 3). Using these control trials as a baseline made it possible to distinguish intention interference from orienting responses to novel or salient stimuli, which never served as PM cues. Commission errors in these studies were often defined as erroneous PM responses in PM$_{\text{REPEATED}}$ trials (Walser et al., 2012), and compared to commission-error rates in control trials to assess commission-error aftereffects (Walser, Goschke, Möschl, & Fischer, 2017; Walser, Plessow, Goschke, & Fischer, 2014). Deviating from the first approach, in this study design participants alternate between active and finished PM phases over the course of several experimental cycles and experimental sessions (e.g., Walser et al., 2012). In order to reduce carry-over effects between cycles, here PM/PM$_{\text{REPEATED}}$ cues change from cycle to cycle. In some studies participants performed another PM task during the finished PM phase (e.g., Walser et al., 2017), but more often, they only performed the ongoing task during the finished PM phase.

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[3] Note that control trials have also been referred to as "oddball trials" (e.g., Walser et al., 2012). Despite the difference in terminology, the key feature of these trials is that they are matched to PM cues in salience and frequency of appearance but were never used as PM cues within the same experiment.

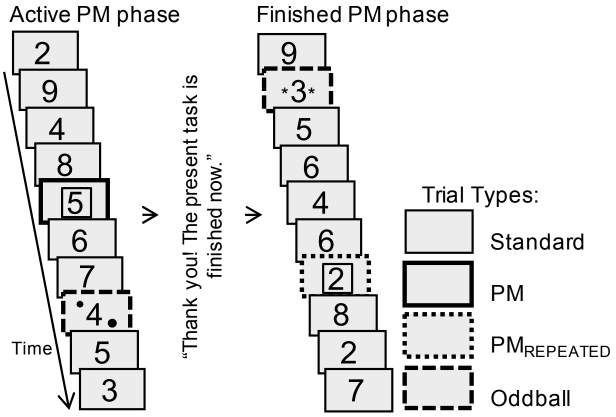

Figure 3. Schematic illustration of an event-based PM task applying an interference logic to assess aftereffects of completed intentions (Walser et al., 2012). Example trials are given for the active PM phase and the finished PM phase. As ongoing task, participants categorized digits as odd or even on all trials expect for PM trials in the active PM phase, which presented a specific symbol among a digit. In these PM trials participants had to perform a specific PM response (e.g., press the spacebar) instead of performing the ongoing task. At the end of the active PM phase, participants were instructed that the PM task had been completed and that they had to perform parity judgments on all subsequent trials in the finished PM phase. Aftereffects were assessed as performance differences (i.e., RTs, ongoing-task errors, commission errors) between $\text{PM}_{\text{REPEATED}}$ and control trials that presented a different symbol than PM trials among a digit (i.e., oddball trials). Standard trials were trials that required an ongoing-task response and did not contain a symbol. Note that framing was not present in the experiments but exclusively serves to illustrate different trial types.

The two main findings from event-based PM studies with $\text{PM}_{\text{REPEATED}}$ cues were slowed and sometimes more erroneous ongoing-task responses in $\text{PM}_{\text{REPEATED}}$ compared to control trials and/or commission errors in $\text{PM}_{\text{REPEATED}}$ trials or shortly thereafter. In some studies, however, commission errors did not occur or were very rare (for modulating variables see Section 2.5). More recently, Anderson and Einstein (2017) reported that aftereffects of completed intentions can manifest in terms of thoughts about the finished PM task or as slowed ongoing-task performance in the first trial following a no-longer-relevant PM cue. Note that in these tasks, in contrast to studies implementing an accessibility logic (Förster et al., 2005; Marsh et al., 1998), slower or more erroneous responses in $\text{PM}_{\text{REPEATED}}$ compared to control trials were interpreted as intention interference that is thought to result from residual activation, re-activation of the completed intention representation or from continued retrieval of the intended action.

These findings—commission errors and/or intention interference—have been repeatedly replicated in laboratory PM tasks (Anderson & Einstein, 2017; Bugg & Scullin, 2013; Pink & Dodson, 2013; Schaper & Grundgeiger, 2017; Scullin & Bugg, 2013; Scullin et al., 2012; Walser et al., 2012), neuroimaging (Beck et al., 2014) and modeling studies (Gilbert et al., 2013). Only a few studies with event-based PM tasks suggest a full, immediate deactivation of completed intentions (A.-L. Cohen et al., 2017; Scullin et al., 2011, 2009). Importantly, in these studies PM and PM$_{\text{REPEATED}}$ cues were non salient and did not pop out from ongoing-task stimuli, which presumably decreased the likelihood of spontaneous intention retrieval (McDaniel et al., 2015). For instance, Cohen et al. (2017) observed no aftereffects with nonfocal PM$_{\text{REPEATED}}$ cues (i.e., double-sided arrows in a display of single-headed arrows). Similarly, Scullin et al. (2011, 2009) found no RT aftereffects or commission errors when PM cues were nonsalient words in an image rating task and aftereffects were measured during a subsequent lexical-decision task.

In contrast to most script-based paradigms, studies using PM$_{\text{REPEATED}}$ cues suggest that intentions are not always *completely* deactivated or inhibited after completion. However, intentions also do not seem to remain *completely* activated after completion. Instead, findings suggest that some deactivation can occur, which allows us to flexibly adapt to novel situations and goals, but also parts of a completed intention seem to remain active.

2.4.3. Intermediate Conclusion

Overall, findings are quite heterogeneous across paradigms and research lines: Script-based paradigms mostly produced findings in favor of a rapid inhibition of completed intentions that may even go below a neutral baseline. Event-based PM paradigms with PM$_{\text{REPEATED}}$ cues mostly produced findings in favor of a continued retrieval of completed intentions. This apparent contradiction may seem quite problematic when assuming that both types of paradigms assess the same underlying concept—the deactivation of completed intentions. However, as we will elaborate more in later sections, script-based paradigms and event-based PM paradigms focus on different components of intention representations. Specifically, script-based paradigms typically assess activation of the content of an intention; event-based PM paradigms that apply an accessibility logic, assess activation of semantic networks related to the PM cue; event-based PM paradigms with PM$_{\text{REPEATED}}$ cues assess activation of PM cues, intended actions and/or the link between PM cues and intended actions. In theory, these components of intention representations might be affected by intention deactivation to different extents and/or become deactivated on different time scales—thus explaining divergent findings across paradigms. In the following sections we will review different putative mechanisms behind aftereffects of completed intentions and the conditions or mechanisms that dictate how much an intention is deactivated after completion.

Table 1

Overview of the Reviewed Studies on the Deactivation of Completed Intentions.

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Script paradigms and related procedures				
Badets, Blandin, Bouquet, and Shea (2006)	Recognition of learned line patterns (motor sequences) / computer instruction (delayed motor intentions)	- Slower recognition of performed line patterns than of control patterns	Inhibition	
Cohen, Dixon, and Lindsay (2005)	Stroop-color naming / computer instruction	- Higher Stroop interference from words that described a performed than a not-performed action	Continued retrieval	- Similar aftereffects in younger and older adults - Aftereffects increased from the first to the second encounter of intention-related items
Marsh, Hicks, and Bink (1998)	Lexical decision task / computer instruction	- Slower lexical decisions on words from a performed than a not-performed action script	Inhibition or deactivation	- Intention-superiority effect for interrupted action scripts: Completed and interrupted portions of an action script showed similar accessibility
Marsh, Hicks, and Bryan (1999)	Lexical decision task / computer instruction	- Slower lexical decisions on words from a performed than a not-performed action script	Inhibition or deactivation	- Inhibition finding also for unrelated completed and cancelled intentions
Meilán (2008)	Recognition of learned verbal scripts / signal	- No difference in recognition RTs on words from a performed and a not-performed action script	Deactivation	
Penningroth (2011)	Lexical decision task / self-initiated time-based cued vs. experimenter cued	- Faster lexical decisions on words from a performed than a not-performed action script	Continued retrieval	- Only state-oriented participants showed a higher sustained intention-superiority effect after intention completion. Action-oriented participants showed comparable accessibility of intention-related and neutral words - Post-completion intention-superiority effect did not differ between self-cued and experimenter-cued tasks
Event-based PM paradigms applying the accessibility logic from script paradigms				
Badets, Albinet, and Blandin (2012)	Verbal-manual task / salient nonfocal PM cues (delayed motor intentions)	- Slower responses for the verbal-manual task in the intention-based than in the stimulus-based group	Inhibition	
Denzler, Förster, and Lieberman (2009)	Lexical decision task / nonsalient focal PM cue	- Stabbing a voodoo doll (intention condition) decreased memory accessibility of aggression-related words more than watching a voodoo doll (no-intention condition)–the decrease of memory accessibility from pre to post measurement was larger after stabbing than after watching	Inhibition	- Completing an aggressive intention reduced memory accessibility of aggression-related words compared to completing a non-aggressive intention, but also led participants to choose pictures with more aggressive content in a picture selection task - Aggressive acts that did not lead to intention completion increased accessibility of aggression-related words

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Denzler, Häfner, and Förster (2011)	Lexical decision task / nonsalient focal PM cue	- Watching a violent computer game with the goal to vent anger (intention condition) produced a stronger decrease in memory accessibility of aggression-related words from pre to post measurement than watching a violent computer game without a goal (no-intention condition)	Inhibition	- Similar findings when participants played a violent computer game to vent anger - Participants who reported a high general tendency to vent their anger, showed reduced relative accessibility of aggression after playing a violent computer game
Förster, Liberman, and Higgins (2005)	Lexical decision task or Stroop-color naming / nonsalient focal PM cue	- Completing a PM task (intention condition) produced a stronger decrease in accessibility of PM-cue related compared to unrelated words (i.e., faster lexical decisions or increased Stroop interference) from pre to post measurement than not performing a PM task (no-intention condition)	Inhibition	- Initial inhibition of completed goals that returns to baseline levels over time - No inhibition effect for unfulfilled goals - Post-completion inhibition increased with increasing expectations to fulfill a goal, increasing goal value or both
Hedberg and Higgins (2011)	Lexical decision task / nonsalient focal PM cue	- Promotion-focused individuals showed a negative correlation between accessibility of PM-cue related words and delay length after intention completion - Prevention-focused individuals showed a positive correlation between accessibility of PM-cue related words and delay length after intention completion	Promotion-focused individuals showed decreased accessibility Prevention-focused individuals showed increased accessibility	
Event-based PM paradigms assessing commission errors and/or interference on PM$_{\text{REPEATED}}$ cues				
Anderson and Einstein (2017)	Lexical decision task / nonsalient focal PM cues	- Slower lexical decisions in PM$_{\text{REPEATED}}$ than in control trials - More frequent thoughts about the finished PM task after PM$_{\text{REPEATED}}$ than after control trials - Commission errors in PM$_{\text{REPEATED}}$ trials - Slower lexical decisions in the first trial after a PM$_{\text{REPEATED}}$ trial than in the first trial after a control trial	Continued retrieval	- Conditions hypothesized to facilitate intention deactivation (clarity, one-off, new PM task) still produced aftereffects: Mainly in slower lexical decisions during or after PM$_{\text{REPEATED}}$ trials and thoughts about the finished PM task after PM$_{\text{REPEATED}}$ trials - Thought probe measures indicated that PM$_{\text{REPEATED}}$ cues often elicit conscious retrieval of an intention
Beck, Ruge, Walser, and Goschke (2014)	Spatial compatibility task / salient focal PM cues	- Slower ongoing-task responses in PM$_{\text{REPEATED}}$ than in ongoing-task trials - Commission errors not analyzed	Continued retrieval	- Similar event-related brain activation in PM$_{\text{REPEATED}}$ and PM trials - Event-related brain activation in PM$_{\text{REPEATED}}$ trials likely reflected processing of PM cues and not merely processing of response conflicts between PM and ongoing-task response - Monitoring related sustained rostro-lateral prefrontal cortex activation stopped after intention completion

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Bugg and Scullin (2013)	Lexical decision task / salient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - RT aftereffects not analyzed	Continued retrieval	- Lower commission-error risk after performing a PM task to 4 PM cues vs. 0 PM cues or cancelling performance of a PM task - Lower commission-error risk with PM$_{\text{REPEATED}}$ cues that were presented (and responded to most times) during active PM phases than with PM$_{\text{REPEATED}}$ cues that were not presented during active PM phases
Bugg, Scullin, and McDaniel (2013)	Lexical decision task / salient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - RT aftereffects not analyzed	Continued retrieval	- Commission-error risk was modulated by the strength of PM-cue–action associations: Higher commission-error risk after encoding PM tasks as an implementation intention than after standard encoding - Commission error risk did not differ between younger and older adults
Bugg, Scullin, and Rauvola (2016)	Lexical decision task / salient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - RT aftereffects not analyzed	Continued retrieval	- Less commission errors and lower commission-error risk after performing a PM task to 4 PM cues than 0 PM cues - More commission errors and higher commission-error risk in older than in younger adults after performing a PM task to 4 PM cues, but no age differences with 0 PM cues - Preparing participants for the occurrence of PM$_{\text{REPEATED}}$ trials (preparatory instructional strategy) did not affect commission errors - Reduced commission-error risk for older adults after practicing to give ongoing-task responses in PM$_{\text{REPEATED}}$ trials (forgetting practice) compared to standard finished instructions - Forgetting practice eliminated age differences in commission-error rates and commission-error risk after performing a PM task to 4 PM cues
Cohen, Gordon, Jaudas, Hefer, and Dreisbach (2017)	Flanker task / nonsalient nonfocal PM cues	- No commission errors in PM$_{\text{REPEATED}}$ trials - No RT difference between PM$_{\text{REPEATED}}$ and control trials	Deactivation	- Absence of monitoring costs after intention completion
El Haj, Coello, Kapogiannis, Gallouj, & Antoine (2018)	Text reading task / nonsalient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - No RT aftereffects analyzed	Continued retrieval	- Higher commission-error rates in older adults with Alzheimer's disease than in healthy older adults - Positive correlation between commission-error rates and Stroop interference and between commission-error and PM omission rates in older adults with and without Alzheimer's disease
Gilbert, Hadjipavlou, and Raoelison (2013)	Letter size discrimination / nonsalient nonfocal PM cues (computational model and simulation)	- Simulating persistence of a PM-task memory trace after intention completion produced slower ongoing-task responses in PM$_{\text{REPEATED}}$ than control trials and commission errors in PM$_{\text{REPEATED}}$ trials	Continued retrieval	- Reducing the strength of PM-cue–action associations reduced commission-error rates but continued to produce slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in control trials

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Pink and Dodson (2013)	Lexical decision task / salient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - RT aftereffects not analyzed	Continued retrieval	- Increased commission-error rates in PM$_{\text{REPEATED}}$ trials under divided attention after completing a habitual PM task
Schaper and Grundgeiger (2017)	Parity judgments / salient focal PM cues	- Commission errors after PM$_{\text{REPEATED}}$ trials: Retrieval and execution opportunity of the PM task were separated by a delay (i.e., delay-execute paradigm)	Continued retrieval	- More commission errors in cancelled than in finished-intention condition - No commission errors in the finished PM phase under divided attention - Commission errors occurred even when PM responses were only possible 45 s after occurrence of a PM$_{\text{REPEATED}}$ cue - RT slowing in first trial after vs. first trial prior to or second trial after a PM$_{\text{REPEATED}}$ trial was similar in cancelled-intention and active-intention group and larger than in completed intention and no-intention group - Interruptions during the delay between PM cue and response opportunity reduced commission-error rates
Scullin and Bugg (2013)	Lexical decision task / salient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - No RT aftereffects analyzed	Continued retrieval	- Commission-error risk increased with higher levels of fatigue (Modulation by cognitive-control availability) - No monitoring costs during finished PM phase - No effects of delay after intention completion on commission errors - Most participants thought that the PM task was finished after finished instruction
Scullin, Bugg, and McDaniel (2012)	Lexical decision task / salient or nonsalient focal PM cues	- Commission errors in PM$_{\text{REPEATED}}$ trials - No RT aftereffects analyzed -	Continued retrieval	- Higher commission-error risk in older than in younger adults - Commission-error risk in older adults correlated with deficits in inhibitory functioning - Higher commission-error risk with salient than with nonsalient PM$_{\text{REPEATED}}$ cues and when ongoing tasks matched between active and finished PM phases
Scullin, Bugg, McDaniel, and Einstein (2011)	Lexical decision task / salient focal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in control trials in older adults but not in younger adults - No commission errors in PM$_{\text{REPEATED}}$ trials	Younger adults: Deactivation Older adults: Continued retrieval	- Stronger response slowing in PM$_{\text{REPEATED}}$ trials in older adults with deficits in inhibitory functioning
Scullin, Einstein, and McDaniel (2009)	Lexical decision task / salient focal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in control trials only with suspended but not with completed intentions - Commission errors not analyzed	Deactivation	- No effects of delay after intention completion on RT aftereffects

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Walser, Fischer, and Goschke (2012)	Parity judgments / salient nonfocal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in oddball or oddball$_{\text{REPEATED}}$ trials - Commission errors in PM$_{\text{REPEATED}}$ trials	Continued retrieval	- Similar response slowing in PMREPEATED cues were verbally described but not shown during encoding and from exemplars of categorical PMREPEATED cues that were not presented during active PM phases - Larger RT aftereffects in finished PM phases with a new PM task than in ongoing-task-only phases - RT aftereffects declined from early to late PMREPEATED encounters
Walser, Fischer, Goschke, Kirschbaum, and Plessow (2013)	Word categorization / nonsalient focal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in ongoing-task trials - Very low number of commission errors (not analyzed)	Continued retrieval	- No effect of acute stress on response slowing in PMREPEATED trials
Walser, Goschke, and Fischer (2014)	Parity judgments / salient nonfocal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in oddball trials - Very low number of commission errors (not analyzed)	Continued retrieval	- RT aftereffects increased after participants described the PM cue compared to after participants read neutral material after intention completion - RT aftereffects decreased after participants performed a demanding backwards-letter span task compared to after they read neutral material - Increased aftereffects for participants low in self-reported action control
Walser, Goschke, Möschl, and Fischer (2017)	Parity judgments or word categorizations / salient/nonsalient and focal/nonfocal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in oddball trials - Higher commission error rates in PM$_{\text{REPEATED}}$ than in control trials	Continued retrieval Deactivation	- When ongoing tasks differed between active and finished PM phases, commission errors and ongoing-task slowing in PMREPEATED versus oddball trials only occurred when a PM task was performed during finished PM phases - RT aftereffects decreased more when new PM tasks required shifting spatial attention to different locations than the finished PM task (different PM-cue categories) than when both PM tasks required attending similar locations (same PM-cue category) - When PM tasks in finished PM phases required a novel PM response, participants gave false-alarm responses with the novel PM response in PMREPEATED trials. They did not make commission errors with the old PM response - Forming a novel dissimilar intention did not lead to overwriting of the completed intention representation
Walser, Plessow, Goschke, and Fischer (2014)	Parity judgments / salient nonfocal PM cues	- Slower ongoing-task RTs in PM$_{\text{REPEATED}}$ than in oddball trials - Very low number of commission errors (not analyzed)	Continued retrieval	- Aftereffects declined over repeated encounters of PMREPEATED trials - Performance differences between PMREPEATED and oddball trials were not related to the delay after intention completion

Study	Ongoing task / Retrieval cue	Main aftereffect results	Interpretation of aftereffects	Additional findings
Free recall of naturalistic intentions				
Freeman and Ellis (2003)	- / free recall in speeded written fluency task	- Younger adults recalled more to-be-completed activities that they intended to perform in the following week than activities they completed one week ago - Older adults recalled a similar amount of to-be-completed and completed activities	Relative inaccessibility / inhibition	- No difference between younger and older adults in the relative inaccessibility (inhibition) of completed intentions
Maylor, Chater, and Brown (2001)	- / free recall in speeded written fluency task	- Less recall of activities that were completed the day/week/year before testing than of to-be-completed activities that were intended to be performed the day/week/year after testing	No interpretation given	
Maylor, Darby, and Della Sala (2000)	- / free recall in speeded written or verbal fluency task	- More recall of completed than to-be-completed activities (intention-inferiority effect) in older adults, no differences in middle aged adults in speeded written fluency task - Only younger adults recalled less completed than to-be-completed activities (intention-superiority effect), but not older adults or dementia patients	Older adults: Reduced facilitation of to-be-completed tasks and/or reduced inhibition of completed tasks	

Note. Many studies postulated both spontaneous retrieval and/or residual activation processes underlying aftereffects of completed intentions or did not exactly separate between those two accounts. Hence, within this table we used the term continued retrieval to refer to both mechanisms.

2.5. Mechanisms Behind Aftereffects of Completed Intentions

As we have shown in the previous sections, research on the fate of completed intentions shows that intention representations seem to be at least partly deactivated after intention completion. However, given the repeated observation of aftereffects, an immediate complete deactivation of intentions seems highly unlikely. This raises the question about the mechanisms behind aftereffects and how intention deactivation occurs.

Regarding aftereffects, so-called retrieval accounts propose that they result from processes that are similar to processes mediating retrieval of to-be-performed intentions. That is, aftereffects might result from spontaneous intention retrieval processes and/or a subsequent failure to control intention execution or from top-down controlled processes of monitoring ongoing-task stimuli for PM cues. By contrast, inhibition accounts propose that aftereffects reflect a below-baseline inhibition of completed intentions that is released over time or through other mechanisms to achieve a return to baseline activation of intention representations. In the following section we will first elaborate these accounts in more detail, then review evidence for each account and lastly discuss if and how these two perspectives can be reconciled.

2.5.1. Retrieval Accounts

Retrieval accounts of aftereffects assume that processes that support intention retrieval during active PM phases continue after intention completion and can incur aftereffects of completed intentions. These retrieval accounts originate primarily from aftereffects research in event-based PM paradigms with $\text{PM}_{\text{REPEATED}}$ cues. Building on prominent theories of PM functioning, at least two different retrieval accounts can be distinguished that offer slightly different explanations for the occurrence of certain types of aftereffects.

First, according to the multiprocess view of PM (e.g., Einstein & McDaniel, 2005; McDaniel et al., 2015), delayed intentions can be retrieved *spontaneously* without preparatory engagement of resource demanding top-down processes prior to processing the retrieval cue. In other words, intentions can “pop” into mind in the presence of a strong retrieval cue. Spontaneous intention retrieval is a probabilistic process that can operate via discrepancy-plus-search processes (Breneiser & McDaniel, 2006; Lee & McDaniel, 2013) or via reflexive-associative-processes (McDaniel, Guynn, Einstein, & Breneiser, 2004;

McDaniel et al., 2015). Therefore, depending on the type of retrieval process, aftereffects may arise through different routes.

According to the discrepancy-plus-search view, individuals are constantly evaluating the fluency of their processing quality (whether they have a PM intention or not). During PM encoding, a cue is changed from a neutral stimulus into one that is associated with an intention, thereby altering its activation level (see intention superiority effect). Therefore, when later processing the PM cue, individuals experience a discrepancy in the quality of their processing (relative to neutral filler stimuli), which subsequently elicits a search in memory for the significance of the stimulus. During this search/attribution process, participants either remember that the stimulus is an active PM cue, remember that the stimulus is an old/finished PM cue, or misattribute their feeling of discrepancy to some other source. Concerning aftereffects, recognizing that PM$_{\text{REPEATED}}$ cues are no longer task relevant and require an ongoing-task response instead of a PM response would yield slowed responses without making a commission error (i.e., RT aftereffects). If the search process is solved wrongly, however, a PM response might be activated, leading to a commission error in PM$_{\text{REPEATED}}$ trials.

According to the reflexive-associative view of spontaneous retrieval (McDaniel et al., 2004, 2015), during encoding, individuals form an association between their intention and possible retrieval cues. In some cases, this association is naturally strong (e.g., remembering to say “thread” when seeing the word “needle”) and in some cases this association is naturally weak (e.g., remembering to say “thread” when seeing the word “sauce”). Even when the association is naturally weak, however, encoding strategies can be used to strengthen the cue–intention link (Bugg, Scullin, & McDaniel, 2013; Scullin, Kurinec, & Nguyen, 2017). The reflexive-associative view is that if the association between a PM cue and an intention is very strong, then later attending to that PM cue will reflexively bring to mind the associated intended action. From this view’s perspective, whether there will be aftereffects depends on the strength of the cue–intention association during the finished PM phase. A reflexive-associative retrieval on PM$_{\text{REPEATED}}$ trials would cause intention interference (Rummel, Einstein, & Rampey, 2012) and might increase risk for commission errors (Bugg et al., 2013).

According to the *dual-mechanisms account of PM commission errors* (Bugg et al., 2016; Scullin & Bugg, 2013), PM$_{\text{REPEATED}}$ cues are thought to initially trigger spontaneous retrieval via discrepancy-plus-search or reflexive-associative processes. These retrieval processes are likely the source of at least some of the RT aftereffects observed. Subsequently, cognitive control is mobilized to reduce or inhibit execution of the completed intention. When cognitive control over intention execution is successful, participants may still show RT aftereffects (intention interference), but avoid making a commission error. By contrast, if the intention is spontaneously retrieved and this cognitive-control mechanism fails, then a commission error will occur.

Second, in terms of *monitoring* accounts of PM performance, intention retrieval is thought to either always (preparatory attentional monitoring theory, e.g., R. E. Smith, 2003) or only under certain conditions (multiprocess view of PM performance, e.g., McDaniel et al., 2015) require the allocation of attention and cognitive resources towards preparatory monitoring for PM cues. From this perspective, aftereffects of completed intentions can result from retrieval of a completed intention, but their occurrence hinges on the presence of top-down controlled monitoring processes during finished PM phases.

In general, besides findings of RT aftereffects and commission errors, retrieval accounts of aftereffects of completed intentions are corroborated by findings of repeated thoughts about the finished PM task after $\mathrm{PM}_{\mathrm{REPEATED}}$-cue encounters (Anderson & Einstein, 2017) and activation in brain areas that are triggered also by PM cues during active PM phases (Beck et al., 2014). Critically, retrieval accounts of aftereffects also make strong predictions about the relation between aftereffects of completed intentions and factors that are known to affect intention retrieval. In the following section, we will review evidence for the specific predictions of each retrieval account.

Likelihood or ease of spontaneous retrieval. First, if aftereffects of completed intentions were the result of *continued spontaneous retrieval*, their occurrence should be influenced by task features that also foster or attenuate spontaneous intention retrieval during active PM phases. Supporting this assumption, Scullin et al. (2012) found that focal and perceptually salient $\mathrm{PM}_{\mathrm{REPEATED}}$ cues (e.g., the word *fish* on a red background screen within a word-categorization task) that are processed rather spontaneously during ongoing-task performance increased commission-error rates and the risk of making a commission error compared to nonsalient $\mathrm{PM}_{\mathrm{REPEATED}}$ cues (black background screen) when ongoing tasks matched between active and finished PM phase. Conversely, only few commission errors were reported when spontaneous retrieval was attenuated by using nonfocal PM cues that required additional processing of ongoing-task stimuli to identify PM cues: For instance, when PM tasks required participants to identify words that contain the syllable *TRA* in a lexical decision task (Beck et al., 2014; A.-L. Cohen et al., 2017; Walser et al., 2012; Walser, Goschke, et al., 2014; Walser, Plessow, et al., 2014; cf. Pink & Dodson, 2013).

Additionally, the occurrence of aftereffects should depend on the integrity or strength of associations between a PM cue, an intended action and the context information that was stored along with stimulus information as part of the PM-task set (Landeira-Fernandez, 1996; Waszak, Hommel, & Allport, 2003). Supporting this assumption, aftereffects were found to be reduced when the ongoing task (i.e., the PM-task context) mismatched between active and finished PM phases (e.g., image-rating task and lexical decision task) compared to when the ongoing task matched in active and finished PM phases (e.g., both lexical decision tasks) (Scullin et al., 2012). In fact, aftereffects were mostly reported in studies using ongoing-task match conditions (Anderson & Einstein, 2017; Beck et al., 2014; Bugg & Scullin, 2013; Bugg et al., 2016; Pink & Dodson, 2013; Scullin & Bugg, 2013; Scullin et al., 2011, 2009; Walser et al., 2012, 2013; Walser, Goschke, et al., 2014; Walser, Plessow, et al., 2014) but not in studies using ongoing-task mismatch conditions (A.-L. Cohen et al., 2017; Walser et al., 2017).

Similarly, aftereffects should be increased by strengthening the PM-cue–intended-action association. This has been demonstrated via implementation-intention encoding (i.e., as an *"if–then"* plan; Bugg et al., 2013) or by repeatedly performing an intended action (i.e., habit formation; Pink & Dodson, 2013; but see Bugg & Scullin, 2013; Bugg et al., 2016 for different findings in a Zeigarnik-like context). Conversely, aftereffects should be reduced when PM$_{\text{REPEATED}}$ cues become associated with ongoing-task responses after intention completion (i.e., doing so is predicted to weaken the cue–intention association). This has been demonstrated after practicing to perform the ongoing task in PM$_{\text{REPEATED}}$ trials (i.e., forgetting practice, Bugg et al., 2016) or after repeated encounters of PM$_{\text{REPEATED}}$ cues during ongoing-task performance (Walser, Plessow, et al., 2014; cf. A.-L. Cohen et al., 2005), which presumably led to a reconfiguration of the PM-task set. Similarly, in a simulation study, commission errors declined after reducing the strength of PM-cue–action associations within a computational model of PM (Gilbert et al., 2013).

Availability of cognitive control over intention execution. Second, according to the *dual-mechanisms account of PM commission errors* (Bugg et al., 2016; Scullin & Bugg, 2013), commission errors should be affected by variables that affect cognitive control over the execution of a completed intention. For one, aftereffects should be especially likely when cognitive control after intention completion is impaired or cognitive-control demands are high. In support of this assumption, several studies found increased commission errors in conditions of limited resources (i.e., divided attention) in the finished PM phase (Pink & Dodson, 2013; cf. Schaper & Grundgeiger, 2017), with fatigued participants (Scullin & Bugg, 2013), or when tasks specifically facilitate attention allocation to $\text{PM}_{\text{REPEATED}}$ cues (Walser et al., 2017). Moreover, older adults who have intact spontaneous retrieval (Mullet et al., 2013) but show impaired inhibitory functioning (e.g., Hasher, Quig, & May, 1997; Lustig, Hasher, & Zacks, 2007; cf. Verhaeghen, 2011; Bugg, 2014 for evidence of spared control with age) were reported to be more at risk of making a commission error to no-longer-relevant PM cues than younger adults—either when PM tasks were active but needed to be suspended briefly (Boywitt, Rummel, & Meiser, 2015) or after PM tasks were finished (Bugg et al., 2016; Scullin & Bugg, 2013; Scullin et al., 2012; cf. Bugg et al., 2013; A.-L. Cohen et al., 2005). Similarly, El Haj, Coello, Kapogiannis, Gallouj, & Antoine (2018) observed that older adults with Alzheimer’s disease made more commission errors than healthy older adults and that commission-error rates in both groups were positively correlated with Stroop interference.

Extending the dual mechanisms account of commission errors, aftereffects in general should be affected by variables that affect cognitive control over response selection; particularly over the response conflict between a to-be-performed ongoing-task response and a no-longer-relevant PM response that arises from intention retrieval in $\text{PM}_{\text{REPEATED}}$ trials. Supporting this idea, Beck et al. (2014) found that during finished PM phases $\text{PM}_{\text{REPEATED}}$ trials incurred transient activation of the rostro-lateral prefrontal cortex that is argued to reflect a mobilization of reactive cognitive control in order to solve response conflicts (Braver, 2012).

Consequently, poor control of response selection would likely not only increase the likelihood for a commission error in a $\text{PM}_{\text{REPEATED}}$ trial, but would also at least increase the amount of time that is needed for correct response selection in a $\text{PM}_{\text{REPEATED}}$ trial. Accordingly, aftereffects should be affected, for instance, by personal dispositions or motivational orientations that affect the mobilization of cognitive control to disengage from a finished intention, which likely alters response selection. Corroborating this notion, prevention-focused individuals who often try to maintain a successfully attained goal state (e.g., Higgins, 1997) showed increased memory accessibility of intention-related semantic networks after intention completion. Conversely, promotion-focused participants who often try to attain a new goal after finishing one (Hedberg & Higgins, 2011) showed decreased memory accessibility after intention completion. In a similar investigation, Walser, Goschke, et al. (2014) found that participants with low self-reported action control (i.e., state orientation; Kuhl, 1992), which had previously been associated with an heightened intention-superiority effect before intention completion (Goschke & Kuhl, 1993), showed increased RT aftereffects of completed intentions as compared to participants with high self-reported action control (i.e., action orientation). Similarly, Penningroth (2011) found a sustained intention-superiority effect after intention completion in participants with low self-reported action control but not in participants with high self-reported action control.

Following the idea that intention deactivation is modulated by cognitive control, aftereffects should be reduced when individuals can mobilize control or prepare for the potential occurrence of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues after intention completion. Regarding effects of preparing for $\mathrm{PM}_{\mathrm{REPEATED}}$-cue encounters, results vary. In one study, Bugg et al. (2016) found that using a preparatory instructional strategy at the end of the active PM phase ("Please note that the target words will appear in the next phase and you may feel the urge to press the 'Q' key in the presence of the target words. That task is finished, however, so we would like you to prepare to avoid pressing 'Q' during the next phase."), did not reduce older adults' susceptibility to making a commission error as compared to a standard finished instruction, and nominally more younger adults made an error with this strategy. Providing participants with opportunities to practice not pressing the Q key in response to target words (i.e., forgetting practice), however, was highly effective in reducing commission errors in older adults, but had no effect in younger adults (because commission-error rates were already on floor). By contrast, Anderson and Einstein (2017) found a nominal decrease in aftereffects when participants verbalized and wrote down the finished PM task instructions or were informed at the beginning of the PM task that the task would be completed as soon as a single PM cue appeared compared to standard finished instructions ("You have now completed the lexical decision task and the pressing the Q key task. Those tasks should not be performed again."). Consequently, Anderson and Einstein (2017) suggested that using some kind of preparatory strategy to elaborate on the completion of a PM task might foster intention deactivation by supporting formation of a stop tag for intention completion (e.g., Bugg & Scullin, 2013).

Monitoring as a prerequisite for intention retrieval. Lastly, when considering the involvement of *monitoring processes* as a precondition for intention retrieval (e.g., R. E. Smith, 2003), aftereffects of completed intentions should only occur in the presence of continued monitoring for PM cues after intention completion. In contrast to this continued-monitoring account of aftereffects, Scullin and Bugg (2013) found commission errors although ongoing-task RTs in the finished PM phase did not differ between an intention and a no-intention condition, suggesting that participants did not continue monitoring for PM cues. Similarly, several studies reported aftereffects despite a decrease of ongoing task RTs (Schaper & Grundgeiger, 2017; Walser et al., 2012, 2013), suggesting that participants disengaged from monitoring after intention completion. In addition, results from a neuroimaging study by Beck et al. (2014) indicate that it is highly unlikely that aftereffects require strategic monitoring for PM cues. The PM task in this study incurred reliable behavioral monitoring costs and elicited activation in the rostro-lateral prefrontal cortex that is assumed to be involved in monitoring (for overviews of neural correlates of PM see Cona et al., 2015; McDaniel et al., 2015). Importantly, however, both the behavioral monitoring costs and activation of the rostro-lateral prefrontal cortex vanished rapidly after the intention had been completed, which indicated that participants ceased to monitor for PM cues. Yet, $\text{PM}_{\text{REPEATED}}$ cues nevertheless produced reliable RT slowing and were associated with event-related activation in brain areas involved in stimulus-driven attention and episodic memory retrieval (ventral parietal cortex, precuneus, posterior cingulate cortex; e.g., Cabeza, Ciaramelli, & Moscovitch, 2012; Summerfield & Egner, 2009). Taken together, this indicates that aftereffects in their study were due to attention capture by $\text{PM}_{\text{REPEATED}}$ cues, which presumably triggered retrieval of the completed intention, in the absence of continued strategic monitoring.

Interestingly, however, recent studies found that performing a novel PM task during finished PM phases increased RT aftereffects and commission error rates (Walser et al., 2012, 2017) or increased intention interference after $\text{PM}_{\text{REPEATED}}$ trials (Anderson & Einstein, 2017). Aftereffects increased even after changing the ongoing task, PM-cue type, PM-cue focality and PM response between novel and finished PM tasks which should have attenuated retrieval of the completed intention (Walser et al., 2017).

In summary, previous findings suggest that the occurrence of aftereffects cannot be explained solely by continued monitoring for PM cues. Yet, in select paradigms, monitoring for novel PM cues during finished PM phases might foster the occurrence of aftereffects.

One potential mechanism behind this might be that forming a PM intention establishes attentional search sets at different levels of abstraction. These search sets bias attention, not only towards features of a specific PM cue and also, on a more abstract level, would establish an unspecific sensitivity to “anything that deviates from the ongoing-task stimuli”. While a PM-cue specific search set would likely be deactivated or altered with a novel PM task, such a general deviant search set would be active, as long as participants perform any kind of PM task. Consequently, when finished PM phases require monitoring for novel PM cues, this general search set would increase the likelihood that $\text{PM}_{\text{REPEATED}}$ cues are automatically categorized as deviations from ongoing task stimuli, which causes at least a temporary interruption of processing even when the ongoing task or stimuli etc. have changed. Additionally, increased sensitivity towards deviant stimuli would increase orienting responses or task interference in deviant control trials when performing a novel PM task during finished PM phases. By contrast, when no PM task has to be performed during finished PM phases, even the very general deviant search set would be deactivated, explaining why aftereffects are significantly reduced under these conditions (Walser et al., 2012, 2017).

Taken together, many findings of aftereffects of completed intentions, particularly in event-based PM paradigms with $\text{PM}_{\text{REPEATED}}$ cues, can be explained by retrieval accounts. That is, the reviewed findings suggest that aftereffects likely result from spontaneous retrieval of a completed intention and its associated task context that is triggered by encounters of $\text{PM}_{\text{REPEATED}}$ cues. The size of aftereffects can be reduced, for instance, by disintegrating or reconfiguring episodic bindings between representations of PM cues, intended actions and their task context as well as by formation of episodic traces or stop tags for having finished a PM task. This seems to require cognitive control over intention execution or more generally over response selection either in a preparatory (before intention reactivation) or a reactive way (after intention reactivation) in order to enable adequate responding to the current task set and to prevent commission errors. Moreover, findings of a modulation of aftereffects by monitoring demands of subsequent activities (i.e., increased aftereffects with novel PM tasks) suggest that establishing an unspecific general deviant search set that biases attention towards discrepant stimuli even after intention completion might be an important factor exacerbating the occurrence of aftereffects—at least when the context information (i.e., ongoing tasks) changes after intention completion.

2.5.2. Alternatives and Extensions of Retrieval Accounts

While the notion of continued intention retrieval is a dominant explanation for aftereffects of completed intentions, some extensions and alternatives of retrieval accounts have been discussed in the literature.

Residual heightened activation of completed intentions. An alternative but not mutually exclusive account of aftereffects of completed intentions builds on Goschke and Kuhl's (1993) intention-superiority hypothesis that intention representations are maintained at a heightened level of sub-threshold activation in long-term memory (see also Marsh & Hicks, 1998). This heightened activation allows a PM cue to stimulate retrieval (perhaps spontaneously; e.g., Einstein & McDaniel, 1996), but a central question has been whether there is persisting or residual activation of the intention representation following intention completion (Walser et al., 2012; Walser, Goschke, et al., 2014). Here residual activation means that intention representations exhibit heightened sub-threshold activation and increased accessibility compared to unrelated memory contents also after intention completion (Penningroth, 2011; see also Cohen et al., 2005 for preliminary support of the residual-activation view). Similar to retrieval accounts of aftereffects, the residual-activation view assumes that intentions continue to be retrieved after their completion, but that this retrieval is also accompanied by heightened activation of intention-related memory contents. Heightened intention activation may be a precondition for reflexive-associative, discrepancy, or other processes that spontaneously trigger intention retrieval both prior to (Goschke & Kuhl, 1993) and after intention completion (Walser et al., 2012; Walser, Goschke, et al., 2014).

Although so far no direct link between intention activation and aftereffects of completed intentions has been shown, several studies on uncompleted or cancelled intentions provide preliminary evidence for such a relationship. Early research by Zeigarnik (1938) showed that participants recalled intended actions that were interrupted more frequently than intended actions that were completed. This finding could be explained by heightened activation of uncompleted intentions (see also Goschke & Kuhl, 1993) that fostered intention retrieval. Similarly, Schult and Steffens (2017) found that increased activation levels of uncompleted intentions were related to increased PM performance, that is, more frequent intention retrieval. These findings suggest a relation between activation levels and the frequency or ease of intention retrieval, at least for uncompleted intentions.

Regarding the relation between intention activation and aftereffects, it has been shown that uncompleted intentions produce stronger aftereffects than completed intentions. For instance, RT slowing in $PM_{REPEATED}$ trials and commission errors increased after suspending (Scullin et al., 2009) or cancelling an intention compared to finishing an intention (Marsh et al., 1999; Schaper & Grundgeiger, 2017; West, McNerney, & Travers, 2007). Similarly, two recent studies found that both commission-error risk and commission-error rates were increased when PM performance had unexpectedly not been possible during an active PM phase (Bugg & Scullin, 2013; Bugg et al., 2016).

Although these findings can be explained by heightened residual intention activation of uncompleted intentions fostering intention retrieval, they could also be explained by a lack of episodic traces for PM performance with suspended, cancelled or uncompleted intentions (i.e., stop-tag account; e.g., Bugg & Scullin, 2013; Giesen & Rothermund, 2014). Future research is necessary, to determine the potential relationship between intention activation and aftereffects of completed intentions, determine whether aftereffects can arise in the absence of residually heightened intention activation and to discriminate effects of residual activation from effects of lacking stop-tag establishing. For this, future studies could, for instance, use action scripts that are performed in response to a PM cue and assess intention activation of script words or semantic associates of a PM cue and at the same time test aftereffects in $PM_{REPEATED}$ trials. Additionally, this might benefit from using neuroimaging or electroencephalography to assess in what ways activation of brain areas related to the content of the intended action and/or the PM cue changes with intention completion.

Output monitoring errors. Recently, commission errors were conceptualized as errors of output monitoring (A.-L. Cohen & Hicks, 2017b)—a person's ability to remember his/her past responses in order to not repeat them (e.g., Koriat et al., 1988). Typically, output-monitoring errors are measured by having participants perform different actions in response to multiple encounters of a PM cue in event-based PM tasks. For instance, Marsh et al. (2007) instructed participants to press one key in response to the first encounter of a PM cue (*first* key) and another key in response to subsequent encounters if they remembered that they already responded to a PM cue (*repeat* key). They found that participants correctly pressed the *first* key during 55–64% of first PM-cue encounters but repeated their first response instead of pressing the *repeat* key in 8-14% of second PM-cue encounters, which suggested that participants sometimes failed to remember their past response (see also Marsh, Hicks, Hancock, & Munsayac, 2002).

Although such findings suggest that commission errors are similar to output-monitoring errors, there are some important differences between output-monitoring paradigms and aftereffect assessments in event-based PM paradigms. That is, although in both paradigms PM responses become irrelevant at a certain point, in paradigms that assess aftereffects of completed intentions participants are explicitly instructed that the PM task has been completed and that both PM cues *and* PM responses are no longer task relevant. In contrast, in typical output-monitoring paradigms it is up to the participants to recognize that they have responded to a PM cue and they have to mark the PM response as irrelevant themselves. Moreover, Scullin and Bugg (2013) showed that 94.3% of participants reported that they believed that the PM task was finished after the finished instructions and also found commission errors after excluding participants with the slightest doubts about the task procedure (see also Scullin et al., 2012). These findings suggest that output-monitoring errors might only account for a small portion of commission errors and other aftereffects in event-based PM paradigms. Future research is necessary to determine in what ways output-monitoring errors contribute to aftereffects of completed intentions.

Confusion about task instructions. Another extension of retrieval accounts can be derived from the question why participants in previous studies sometimes failed to successfully deactivate an intention following its completion. It is conceivable that participants were merely confused about the finished instructions, which might lead them to continue performing the PM task during the finished PM phase. Although plausible, empirical evidence does not support this reasoning. Anderson and Einstein (2017), for example, recently showed that no participants reported thoughts about a finished PM task at the beginning of the finished PM phase. Similarly, Scullin et al. (2012) still observed commission errors when they excluded participants who indicated confusion about the PM task, showed poor recall of the PM task or gave false-alarm PM responses during the active PM phase from their analyses. Moreover, commission errors are likely not simply slip-ups made possible by the experimental apparatus. That is, commission errors were reported even when participants were prepared that $\text{PM}_{\text{REPEATED}}$ cues would appear during the finished PM phase (e.g., Bugg et al., 2016) or when participants needed to overtly move one hand from the lap to the keyboard in order to make a commission error (Scullin et al., 2012). Therefore, although confusion about finished instructions, the proximity or similarity of response buttons for ongoing-task and PM-task responses might contribute to the occurrence of aftereffects, they likely are not the sole reason for aftereffects to occur.

Stimulus-triggered retrieval of specific stimulus–response links. Aftereffects of completed intentions might be the result of direct stimulus–response links that are acquired during task performance (Walser et al., 2012). That is, during the active PM phase participants are often required to respond to multiple occurrences of PM cues with a simple key press. Each response results in sensorimotor learning and the development of a shared episode or stimulus–response link between the *specific* PM cue and the corresponding response (Abrams & Greenwald, 2000; Hommel, 1998; Logan, 1988; Neumann & Klotz, 1994). Similarly, this link may be established already at encoding of a PM task as a temporarily instructed binding between PM cue and intended action (Cohen-Kdoshay & Meiran, 2007; Gollwitzer, 1999; Meiran, Pereg, Kessler, Cole, & Braver, 2015). Consequently, encountering $\mathrm{PM}_{\mathrm{REPEATED}}$ cues may trigger retrieval of the stimulus–response episode and thus prime the acquired associated response resulting in response slowing or commission errors.

In contrast to this assumption, it was shown that the occurrence of aftereffects does not seem to require having performed the intended action in response to a *specific* PM cue or being instructed to respond to a specific PM cue. That is, RT aftereffects could also be observed for novel exemplars of categorical PM cues that were not presented during the PM task (Walser et al., 2012; Walser, Plessow, et al., 2014) and commission errors and RT aftereffects did not differ between PM cues that were only presented during PM-task encoding and those that were presented at encoding *and* during the PM task where they were responded to on most occasions (Anderson & Einstein, 2017; cf. Bugg & Scullin, 2013; Bugg et al., 2016). These findings suggest that aftereffects can stem from instructed PM-cue–action links that seem to generalize beyond specific PM-cue exemplars towards more abstract stimulus representations (i.e., concepts representing a stimulus category), as has also been observed in active PM phases (Cook, Marsh, Hicks, & Martin, 2006; Ellis & Milne, 1996; cf. Mullet et al., 2013).

Lack of episodic traces for intention completion. According to a stop-tag theory of intention deactivation (Bugg & Scullin, 2013; Bugg et al., 2016), aftereffects might arise from a lack of episodic traces for having performed an intended action (e.g., Hommel, 2009; Hommel, Müsseler, Aschersleben, & Prinz, 2001). The idea is that performing an intended action establishes episodic traces about PM-task completion and binds a stop-tag to the representation of a PM$_{\text{REPEATED}}$ cue that should aid in successful intention deactivation. Consequently, aftereffects should be reduced when active PM phases offer opportunities to perform the intended action compared to when intention completion is not possible. Supporting this notion, aftereffects were found to be increased when PM-tasks were cancelled before they could be performed (e.g., Bugg et al., 2016) and commission errors occurred more frequently when PM cues were only presented at PM-task encoding but not in the active PM phase, compared to when PM cues were presented during encoding and in the active PM phase (Bugg & Scullin, 2013; Bugg et al., 2016; cf. Anderson & Einstein, 2017).

Lack of time or cognitive resources for complete intention deactivation. One could argue that a complete intention deactivation might require time or the availability of cognitive resources after intention completion. Initial evidence suggested that time is a crucial factor for successful intention deactivation. Hedberg and Higgins (2011), for example, found that aftereffects decreased with increasing delays after intention completion. This effect, however, was restricted to participants who were trying to disengage from successfully attained goal states (promotion-focused participants; e.g., Higgins, 1997). Along the same lines, Förster and colleagues (2005) found that lexical-decision slowing or Stroop-interference reduction on PM-cue related versus control words decreased over time in intention compared to no-intention groups, which suggested that intention inhibition was released over time. Crucially, however, this effect may have been confounded with repeated encounters of intention-related stimuli that has been shown to reduce aftereffects in event-based PM tasks. For example, contrasting the temporal delay after intention completion and the frequency of PM$_{\text{REPEATED}}$ -cue encounters showed that only the frequency of PM$_{\text{REPEATED}}$-cue encounters, but not the temporal manipulation reduced the size of aftereffects of completed intentions (Walser, Plessow, et al., 2014). Several additional studies reported no effects of introducing a delay between active and finished PM phases on aftereffects (Scullin & Bugg, 2013; Scullin et al., 2011).

Regarding the interplay of available time and cognitive resources for intention deactivation, Penningroth (2011) speculated that successful intention deactivation might require postactional processing to actively disengage from a completed intention. This idea is built on suggestions by Beckmann (1994), who found that ruminations about performance failures could effectively be reduced when participants were provided with ample time and resources to engage in a postactional evaluation of their performance and were presented with cues for a novel activity. According to this account, aftereffects should increase when participants perform an unrelated attention-demanding task immediately after intention completion that attenuates postactional processing. Interestingly, however, Walser, Goschke, et al. (2014) found that aftereffects actually *decreased* when participants performed a highly demanding working memory task compared to when they performed an undemanding letter-reading control task. Their findings suggest that postactional processing alone is likely not enough to deactivate successfully completed intentions. Instead, aftereffects seem to be sensitive to working memory demands or novel memory contents.

Lack of novel memoranda to overwrite completed intention representations. Aftereffects might arise due to a lack of novel information that could overwrite the finished intention representation. According to this view, the encoding of novel memory representations after intention completion might foster intention deactivation and reduce aftereffects by destabilizing and overwriting parts of the intention representation (e.g., Walser, Goschke, et al., 2014). Interestingly, research suggests that completed intentions cannot be overwritten by forming a novel PM intention. Specifically, three studies showed that simply performing a novel PM task after intention completion compared to performing an ongoing task alone did not reduce aftereffects; each time, the new task exacerbated aftereffects (Walser et al., 2012, 2017; see also Anderson & Einstein, 2017). Yet, it might be possible to partially overwrite intention representations by forming novel unrelated memoranda, given that performing a demanding working-memory task after intention completion has been shown to reduce aftereffects (Walser, Goschke, et al., 2014; see also Schaper & Grundgeiger, 2017, for reduced commission errors after filling the response delay during a delay-execute PM task with an unrelated task). However, this overwriting might not be utilized as a deactivation strategy. That is, when explicitly asking participants if they tried to deactivate the intention by thinking of a new task almost every participant indicated that they did not use that strategy at all (Bugg et al., 2016).

2.5.3. Inhibition Account

The inhibition account of aftereffects assumes that the activity level of an intention representation is actively reduced upon intention completion. This account originates primarily from research with script-based paradigms and event-based PM paradigms applying an accessibility logic. An extreme interpretation of this account is that the activation level of intention-related memory contents might even drop below a baseline of unrelated memory contents that were recently acquired. Within this interpretation, inhibition represents an active suppression of intention-related memory contents. As a consequence, aftereffects show up in response slowing in facilitation paradigms (e.g., slowed lexical-decision performance) or decreased interference in interference paradigms (e.g., less Stroop-color naming interference) in intention-related trials compared to control trials. A less extreme interpretation of the inhibition account is that inhibition reduces the activation level of a completed intention towards a neutral baseline but not necessarily below it. This interpretation fits with findings of reduced intention activation after completion compared to before intention completion (e.g., Förster et al., 2005) and could also account for findings of a return to baseline activation of completed intentions (Meilán, 2008). In this sense inhibition is seen less as an active suppression but more as an (active) deactivation process that after intention completion might gradually decrease the activation level of intention representation.

The inhibition account has received much attention in the literature as it contains a plausible functionality: Inhibition of completed intentions may facilitate establishing novel intention representations (Förster, Liberman, & Friedman, 2007; Förster et al., 2005; Marsh et al., 1998; see also Allport et al., 1994; Mayr & Keele, 2000). When participants were asked to report the strategy, they used to deactivate intentions, mentally suppressing the finished task was the second most frequent strategy reported (following compartmentalized by considering the finished block as a new context; Bugg et al., 2016). Furthermore, inhibition was regarded as one of several central features that could distinguish memory-activation of goal representations from effects of semantic priming (Förster et al., 2007).

Although post-completion intention inhibition is a plausible mechanism that seems to be supported reasonably well, some findings stand in stark contrast with assumptions of a *complete* inhibition of *all components* of completed intentions. First, if completed intentions became inhibited, accessibility of intention-related stimuli should be smaller than accessibility of familiar or primed stimuli (Förster et al., 2007). In contrast to this assumption, RTs in $\text{PM}_{\text{REPEATED}}$ trials were actually found to be larger than RTs in control trials that were made familiar through repeated presentation to participants during active PM phases (i.e., $\text{oddball}_{\text{REPEATED}}$ trials in Walser et al., 2012, Exp. 2). Second, across different paradigms, similar findings were regarded as evidence either for or against an inhibition of completed intentions: Performance costs in trials that presented intention-related stimuli (i.e., words describing intended actions; words semantically related to PM cues; $\text{PM}_{\text{REPEATED}}$ cues) were interpreted as evidence for intention inhibition (Förster et al., 2005; Marsh et al., 1998), but were also taken as evidence for intention interference due to continued retrieval (Anderson & Einstein, 2017; Beck et al., 2014; Scullin et al., 2011; Walser et al., 2012). Third, and perhaps most critically, when assuming a *complete* inhibition of all components of a completed intention, it would not be possible to make commission errors in response to $\text{PM}_{\text{REPEATED}}$ cues, since the cognitive intention representation and the intended action would not be retrievable.

2.5.4. Inhibition and Intention Retrieval: Dichotomy or Continuum?

In light of the divergent findings between script-based and event-based PM paradigms two questions arise: What are the reasons for the disparity between the inhibition account and its alternatives? Can the notions of intention inhibition and continued retrieval fit together? In our opinion, some explanations for this are conceivable.

First, the direction and mechanism of aftereffects might depend on which component of a cognitive intention representation each experimental approach focuses on. More specifically, testing aftereffects of memory contents that are semantically related to a completed intention (like in studies that suggest intention inhibition) presumably captures the accessibility or retrieval of the semantic content of an intention. Testing aftereffects of no-longer-relevant PM cues (like in studies suggesting continued intention retrieval), on the other hand, presumably captures accessibility or retrieval of PM-cue representations, intended actions or PM-cue–action links. Recent studies provide evidence for a dissociation of multiple components of intention representations (e.g., Burgess et al., 2011; A.-L. Cohen, West, & Craik, 2001; Momennejad & Haynes, 2012). Hence, it is feasible that intention deactivation or inhibition may have diverging effects on these components and act on different time scales for each component. That is, PM-cue representations, intended actions and their association might be deactivated or inhibited slowly and cause intention interference due to continued intention retrieval. Semantic networks that are associated with the PM cue or the content of an intended action, however, might become inhibited more rapidly after intention completion.

Second, findings that suggest an inhibition of completed intentions (Förster et al., 2005; Marsh et al., 1998) may be re-interpreted as evidence for continued intention retrieval after completion. Most studies on the accessibility of intention-related memory contents that favored an inhibition account employed lexical decision tasks (e.g., Marsh et al., 1998). In these tasks both intention inhibition and spontaneous retrieval of completed intentions could lead to prolonged RTs to intention-related stimuli: Spontaneous retrieval of a completed intention would interfere with and impair lexical decision performance, whereas intention inhibition would reduce accessibility of intention-related memory contents and also impair lexical decision performance. Some preliminary support for this idea can be derived from findings that increasing the expectancy or motivation to detect a PM cue produced an increase of "intention inhibition" (Förster et al., 2005 Exp. 4–6) that seems to be similar to findings of increased aftereffects with stronger PM-cue–action associations due to implementation-intention encoding (Bugg et al., 2013) or habit creation (Pink & Dodson, 2013). Similarly, aftereffects in response to stimuli that are semantically related to a PM cue might actually reflect spontaneous intention retrieval instead of inhibition, given that previous studies showed that intention retrieval may also occur in response to semantic associates of a specific PM cue (Cook et al., 2006; Ellis & Milne, 1996; cf. Mullet et al., 2013). Note, however, that this reinterpretation is not unproblematic as it conflicts with observations of decreased accessibility of intention-related memory contents after compared to before intention completion in a Stroop task (Förster et al., 2005)[4] and with a finding of increased accessibility of intention-related memory contents after intention completion in a lexical-decision task (Penningroth, 2011). Moreover, this re-interpretation implies that the notion of an inverse relation between RT and activation in memory (Ratcliff & McKoon, 1978) should hold prior to intention completion, but should be reversed after intention completion. This means the notion that increased RTs on intention-related stimuli in facilitation paradigms reflect decreased activation might only hold for stimuli/concepts that are relevant for current task performance. When stimuli become irrelevant or are associated with an action or a *stop-tag* due to intention completion (Bugg & Scullin, 2013), the RT–activation relation might change so that increased RTs on intention-related stimuli in facilitation paradigms reflect increased activation.

Third, it is conceivable that our cognitive system generally inhibits intention representations after completion of a PM task but that this inhibition might sometimes

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[4] In this study (Förster et al., 2005, Exp. 2), Stroop interference on PM-cue-related words relative to control words decreased from before to after intention completion, which was interpreted as evidence for post-completion inhibition. Note, however, that after intention completion, interference was still stronger from PM-cue-related words compared to control words, which could also be interpreted as evidence for continued intention retrieval or residual intention activation.

completely or partially fail, which would result in effects of residual activation or continued intention retrieval (e.g., commission errors) due to *incomplete* intention inhibition. Thus, it is conceivable that findings of residual activation or continued retrieval might only stand in contrast with findings of intention inhibition when assuming a perfect inhibition mechanism. Preliminary support for this idea stems from findings that suggest that aftereffects or activation of successfully completed intentions is often lower than activation of suspended intentions that are yet-to-be-completed (Scullin et al., 2009) and lower than activation of intentions that were cancelled before performance had started (Bugg & Scullin, 2013; Marsh et al., 1999; Schaper & Grundgeiger, 2017; West et al., 2007).

2.5.5. Conclusion

In this chapter, we reviewed empirical studies on aftereffects of completed intentions. Our motivation for this was to provide an up-to-date overview of experimental procedures and main results of aftereffect research, a synopsis of current theories about the source of aftereffects as well as to highlight commonalities and differences between different lines of research in this field. Our review shows that intention deactivation does not operate like a light switch. That is, we neither experience massive interference from finished tasks all the time nor do we experience no aftereffects at all. Instead, intention deactivation is better conceptualized as a continuum of decreasing activation levels below that present before the intention has been completed, and ultimately reaching baseline levels akin to neutral stimuli. This general process might not be linear, it may sometimes be faulty and its time course may be accelerated or decelerated by a diversity of factors.

Importantly, understanding how intention deactivation occurs requires understanding the cognitive factors that underlie encoding, maintenance, retrieval and execution of intended actions as well as their interplay and cognitive control over them. Here, for instance, specific features of retrieval cues (e.g., salience), task instructions and the PM-task context play an important role for the way these factors affect intention retrieval before and after completion. A firm understanding of these factors allows us to predict the time course of deactivation as well as conditions in which intention deactivation will be maximal or minimal. So far, the reviewed literature begins to illuminate a profile of when intention deactivation is most difficult and people are likely most at risk to experience interference from completed intentions or to make commission errors: (a) When there is a strong link between the PM cue and intended action, an increased readiness for intention retrieval and possibly a high activation level of the intention representation; (b) when encoding of finished instructions, episodic memory for intention completion and control over response selection and/or intention execution are impaired; (c) when PM/PM$_{\text{REPEATED}}$ cues are extremely salient and we remain in a PM-task context, or when there is a high overlap between subsequent tasks and the original PM-task context. However, although each of these factors likely exacerbates the occurrence of aftereffects and commission errors in particular, whether or not commission errors occur at all might nevertheless depend on a combination of multiple factors. For instance, Walser et al. (2012) observed nearly no commission errors when using salient nonfocal PM cues. Similarly, even with a high context overlap between active and finished PM phase and focal PM cues, Scullin et al. (2012) found more commission errors when PM-cues were salient than when they were nonsalient. Future research is required to identify conditions under which commission errors reliably occur in most participants and also to determine how tasks should be designed to support rapid intention deactivation.

As we showed in this review, the deactivation of completed intentions is an essential aspect of successful goal-directed behavior. Failures of intention deactivation likely contribute to failures of goal directedness in everyday life, like overmedication, and also show parallels to phenomena like intrusions and ruminations (Ehlers & Clark, 2000; Whitmer & Gotlib, 2012). Surprisingly, despite the importance of this topic, only relatively few studies have been conducted in this field. Nevertheless, the overall picture across several research approaches suggests that completed intentions neither persist completely nor are deactivated immediately. Instead, intention deactivation could be considered as a dynamic process, depending on a variety of modulating factors. Identifying and specifying these factors could help us gain a better understanding of intention deactivation and could also help improve predictions about the conditions under which aftereffects are particularly likely or unlikely. As a next step in this direction, the following study tested the effects of transient cognitive-control demands during encounters of PM$_{\text{REPEATED}}$ cues on intention deactivation in younger and older adults.

3. Study 1 – Effects of Age and Cognitive-Control Availability on the Deactivation of Completed Intentions

3.1. Abstract

In everyday life, we frequently postpone intended actions (prospective memory; PM), but also need to deactivate completed intentions in order to flexibly adapt to subsequent activities. Recent findings of an increased risk to erroneously repeat completed actions (i.e., commission errors) in older adults with cognitive-control deficits suggest that successful intention deactivation hinges on the availability of cognitive control. It is unclear, however, whether transient availability of cognitive control during encounters of no-longer-relevant PM retrieval cues ($\text{PM}_{\text{REPEATED}}$ cues) is critical for intention deactivation. Here we investigated this with varying levels of cognitive-control availability during $\text{PM}_{\text{REPEATED}}$-cue encounters in older and younger adults. Replicating previous findings, in Experiment 1 we found that older adults exhibited a tendency for impaired intention deactivation in terms of nominally higher performance costs in $\text{PM}_{\text{REPEATED}}$ trials and a higher commission-error risk than younger adults. While performance costs and commission-error rates in $\text{PM}_{\text{REPEATED}}$ trials did not differ between extremes of cognitive-control availability, slightly loading cognitive-control resources drastically reduced performance costs. Importantly, this effect was not replicated with a more fine-grained variation of cognitive-control availability in Experiment 2, in which we exclusively tested younger adults. These findings suggested that intention deactivation is not modulated by transient cognitive-control availability. Surprisingly, however, we also found that the rather low number of commission errors in Experiment 2 even dropped with decreasing cognitive-control availability. We discuss conditions under which transient cognitive-control availability may affect intention deactivation and review alternative sources of age effects on intention deactivation besides global cognitive-control impairments.

3.2. Introduction

Several times throughout a day, we form intentions that are postponed until the right circumstances are met to perform them (Kliegel & Martin, 2003). This ability, known as prospective memory (PM), comes into play, for instance, when we plan to give a friend a call at 6 p.m. (time-based PM) or when coming home (event-based PM). While PM enables us to perform everyday tasks like these, deactivating intentions after their completion makes it possible to flexibly re-adjust behavior according to novel intentions or current situational demands (Goschke, 2003; Goschke & Bolte, 2018; Mayr & Keele, 2000). Surprisingly, completing an intention does not necessarily lead to a direct deactivation of the intention representation, but can impair subsequent task performance and even trigger erroneous repetitions of completed intentions (commission errors) when encountering no-longer-relevant PM cues ($\text{PM}_{\text{REPEATED}}$ cues) that previously signaled the opportunity to retrieve an intention (Anderson & Einstein, 2017; Pink & Dodson, 2013; Scullin et al., 2012; Walser et al., 2012; cf. A.-L. Cohen et al., 2017; Scullin et al., 2009). Previous findings suggest that especially older adults exhibit difficulties in disengaging from no-longer-relevant tasks (e.g., Boywitt et al., 2015; Scullin et al., 2011), which in some cases might translate to commission errors like accidental overmedication (Gray et al., 2001; Kimmel et al., 2007). Importantly, the mechanisms behind these so-called aftereffects of completed intentions and successful intention deactivation are still not well understood.

In the present study, we aimed at elucidating the role of cognitive-control availability during encounters of $\text{PM}_{\text{REPEATED}}$ cues for intention deactivation. To this aim, we investigated aftereffects of completed intentions under varying degrees of cognitive-control demands for ongoing-task processing during $\text{PM}_{\text{REPEATED}}$ -cue encounters. Additionally, to further clarify under which conditions aging may affect intention deactivation we investigated the potential interplay between cognitive-control availability and aging effects on the deactivation of completed intentions.

While intention deactivation does not seem to simply rely on availability of time (Scullin & Bugg, 2013; Walser, Plessow, et al., 2014) or cognitive resources after intention completion (Walser, Goschke, et al., 2014), as was suggested by earlier work (Beckmann, 1994; Penningroth, 2011), more recent findings suggest that successful intention deactivation hinges on cognitive-control availability during encounters of $\text{PM}_{\text{REPEATED}}$ cues. More specifically, according to a dual mechanisms account, commission errors are the result of an initial spontaneous and almost automatic retrieval of an intention in response to PM$_{\text{REPEATED}}$ cues that is followed by a failure to deactivate or inhibit retrieval of the no-longer-relevant intention via cognitive-control mechanisms (Bugg et al., 2016; Scullin & Bugg, 2013; see also Meiser & Rummel, 2012).

Importantly, even in the absence of a commission error, failures of complete intention deactivation frequently lead to slowed or more erroneous ongoing-task performance in PM$_{\text{REPEATED}}$ trials compared to neutral control trials (Scullin et al., 2011; Walser et al., 2012). One source of these aftereffects is that encountering PM$_{\text{REPEATED}}$ cues triggers a conflict between the to-be-performed ongoing-task response and the no-longer-relevant PM response. This, for instance, was demonstrated by a neuroimaging study (Beck, Ruge, Walser, & Goschke, 2014): PM$_{\text{REPEATED}}$ cues triggered increased activation of the anterior cingulate cortex and transient activation of rostrolateral prefrontal cortex. The authors argued that this pattern reflects mobilization of reactive control by a response conflict (Braver, 2012). Since resolving this conflict takes time and cognitive resources, Beck et al. (2014) observed response slowing in PM$_{\text{REPEATED}}$ trials.

The notion of a cognitive-control dependent intention deactivation is corroborated by several additional findings: First, commission errors were observed more frequently under conditions that tax cognitive control after intention completion, like divided attention (Boywitt et al., 2015; Pink & Dodson, 2013; cf. Schaper & Grundgeiger, 2017) and fatigue (Scullin & Bugg, 2013) or under increased intention-deactivation demands due to implementation-intention encoding of PM tasks (Bugg et al., 2013) or a lack of retrieval opportunities for intended actions (Bugg & Scullin, 2013; Bugg et al., 2016). Second, increased RT aftereffects were also linked to impaired cognitive flexibility, as has been observed in state-oriented individuals (Walser, Goschke, et al., 2014; see also Penningroth, 2011). Third and most prominently, the notion of a control-dependent intention deactivation is corroborated by findings of older adults being more prone to making a commission error after intention completion than younger adults (Bugg et al., 2016; Scullin & Bugg, 2013; Scullin et al., 2012, 2011; cf. Bugg et al., 2013; A.-L. Cohen et al., 2005) or older adults showing RT aftereffects under certain conditions where younger adults did not (Scullin et al., 2011). The increased frequency and magnitude of aftereffects of completed intentions in older adults has mostly been attributed to a supposed age-related decline in inhibitory cognitive-control functions (e.g., Hasher et al., 1997; Lustig et al., 2007; cf. Verhaeghen, 2011; Bugg, 2014). In some studies it was also linked to impaired performance in Stroop color naming, Trail Making Test B and Wisconsin Card Sorting Test in older but not younger adults (Scullin & Bugg, 2013; Scullin et al., 2012).

Although the above findings seem to corroborate the dual-mechanisms account of commission errors they do not provide unambiguous support for a general cognitive-control dependence of intention deactivation, given the diversity of variables that seem to affect intention deactivation. First, the effects of aging, divided attention, implementation-intention encoding, fatigue and state orientation can be considered relatively long lasting and may thus not only affect intention deactivation, but also PM performance and task performance after intention completion. This in turn might influence aftereffects of completed intentions. Second, it is debatable whether the observed modulators of intention deactivation primarily target cognitive-control availability: At least with active intentions, divided attention manipulations, for instance, have been shown to affect spontaneous intention retrieval but not cognitive control over executing intended actions (Harrison, Mullet, Whiffen, Ousterhout, & Einstein, 2014). That is, demanding divided attention task (random number generation) only impaired PM performance when intention retrieval was presumably not fully automatic—specifically, with nonsalient PM cues. With salient PM cues, however, divided attention manipulations had no effect on PM performance. Hence, increased aftereffects under divided attention might not reflect impaired inhibition of no-longer-relevant intended actions but rather impaired intention retrieval. Most importantly, so far there is no direct test of a potential cognitive-control dependency of intention deactivation and little is known about the effects of cognitive-control availability during encounters of PM$_{\text{REPEATED}}$ cues and intention deactivation. Hence, based on previous studies it is not only difficult to pinpoint the mechanism(s) responsible for successful intention deactivation. It is also unclear whether intention deactivation is simply affected by general task demands prior to or after intention completion or whether it is particularly susceptible to cognitive-control availability during encounters of PM$_{\text{REPEATED}}$ cues, as suggested by the dual mechanisms account of PM commission errors (Bugg et al., 2016).

3.3. The Present Study

Here, we directly tested the effects of transient cognitive-control availability during PM$_{\text{REPEATED}}$-cue encounters on the deactivation of completed intentions. For this, in two experiments we parametrically varied trial-by-trial cognitive-control demands during task performance and presented PM$_{\text{REPEATED}}$ cues in trials that induced varying degrees of conflict in stimulus processing (e.g., Fan, 2014) (Figure 4). This allowed us to elucidate the relationship between the transient availability of cognitive control after intention completion and intention deactivation beyond the two-step manipulations of cognitive-control demands in previous studies (e.g., Bugg & Scullin, 2013; Bugg et al., 2013, 2016). Additionally, we aimed at separating potential effects of transient versus enduring cognitive-control demands on intention deactivation to further investigate under which conditions aging might affect intention deactivation. To this aim, in Experiment 1, we compared the effects of trial-by-trial cognitive-control demands on aftereffects of completed intentions between younger and older adults. In Experiment 2, we assessed only younger adults and extended the control-demand manipulation to allow for a more fine-grained analysis of cognitive-control-availability effects on aftereffects of completed intentions.

To reduce the risk of confounding potential effects of age or cognitive-control availability with effects of PM performance differences on aftereffects of completed intentions (Bugg & Scullin, 2013; Bugg et al., 2016), we used a design that should reliably produce both high PM performance and strong aftereffects of completed intentions: First, we used salient PM cues (i.e., red symbols) that have been shown to result in high PM performance (Beck et al., 2014; Scullin et al., 2012), reduce age differences in event-based PM performance (Kretschmer-Trendowicz & Altgassen, 2016; Rendell, McDaniel, Forbes, & Einstein, 2007) and seem to reliably produce strong aftereffects of completed intentions (Beck et al., 2014; Scullin et al., 2012). Second, our task instructions strongly emphasized the importance of the PM task during task instructions to foster PM performance (Walter & Meier, 2014). Third, to increase the size of aftereffects of completed intentions, we used the same ongoing task during PM-task performance and aftereffects measurement (Scullin et al., 2012).

Both experiments employed a paradigm during which participants performed multiple cycles of a PM block and a subsequent test block (see also Walser et al., 2012). Participants’ ongoing task was to judge the direction of the majority of horizontal arrows that were presented in a circular display (majority function task; Fan, Guise, Liu, & Wang, 2008; Wu, Dufford, Mackie, Egan, & Fan, 2016) (Figure 4). In PM blocks, participants received additional instructions to press the spacebar instead of making directional judgments in response to a pre-specified PM cue. In subsequent test blocks that served to measure aftereffects of completed intentions, no-longer-relevant PM cues from the completed PM task ($\mathrm{PM}_{\mathrm{REPEATED}}$ cues) were presented as irrelevant distractors (e.g., Anderson & Einstein, 2017; Pink & Dodson, 2013; Scullin et al., 2012; Walser et al., 2012). The primary measures of interest were aftereffects of completed intentions during test blocks in terms of commission errors and ongoing-task performance costs (i.e., response slowing and increased error rates) during $\mathrm{PM}_{\mathrm{REPEATED}}$ compared to control (i.e., oddball) trials that served as a baseline to assess orientation reactions to novel stimuli (Walser et al., 2012).

To induce different degrees of cognitive-control availability during processing of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues, we manipulated the ratio of arrows pointing to the same or opposite direction during the ongoing majority-function task (e.g., trials in ratio 3:2 comprised 3 left and 2 right arrows; Figure 4B). This manipulation of arrow ratio goes beyond the qualitative manipulation of cognitive-control demands (conflict vs. no-conflict) in typical conflict tasks like Stroop color naming (Stroop, 1935) or Flanker tasks (Eriksen & Eriksen, 1974) in that it allows to induce different degrees of conflict and cognitive-control demands (i.e., computational load, Fan et al., 2008). We reasoned that inducing this kind of computational load in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials should mobilize and bind cognitive-control resources for ongoing-task performance (Goschke & Dreisbach, 2008; Scherbaum, Fischer, Dshemuchadse, & Goschke, 2011) and draw resources away from processes required for intention deactivation. In line with previous findings of conflict-strength dependent adjustments of cognitive-control functions (e.g., Wendt, Kiesel, Geringswald, Purmann, & Fischer, 2014), this control mobilization should be proportionate to the degree of conflict induced by ongoing-task stimuli. Accordingly, we hypothesized that if intention deactivation requires available cognitive control during $\mathrm{PM}_{\mathrm{REPEATED}}$ cue encounters, aftereffects of completed intentions in terms of commission errors and/or performance costs in $\mathrm{PM}_{\mathrm{REPEATED}}$ compared to oddball trials should rise with increasing cognitive-control demands on ongoing-task performance (i.e., conflict strength) in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials.

Additionally, based on previous findings of impaired intention deactivation in older adults (Bugg et al., 2016), in Experiment 1, we expected a main effect of age with increased aftereffects of completed intentions in older compared to younger adults. Furthermore, since age effects on commission errors have been shown to increase under high demands on cognitive control (Bugg et al., 2016) and were linked to impaired cognitive-control functioning in older adults (Scullin et al., 2012, 2011), we assumed that aging would exacerbate effects of cognitive-control demands on aftereffects of completed intentions.

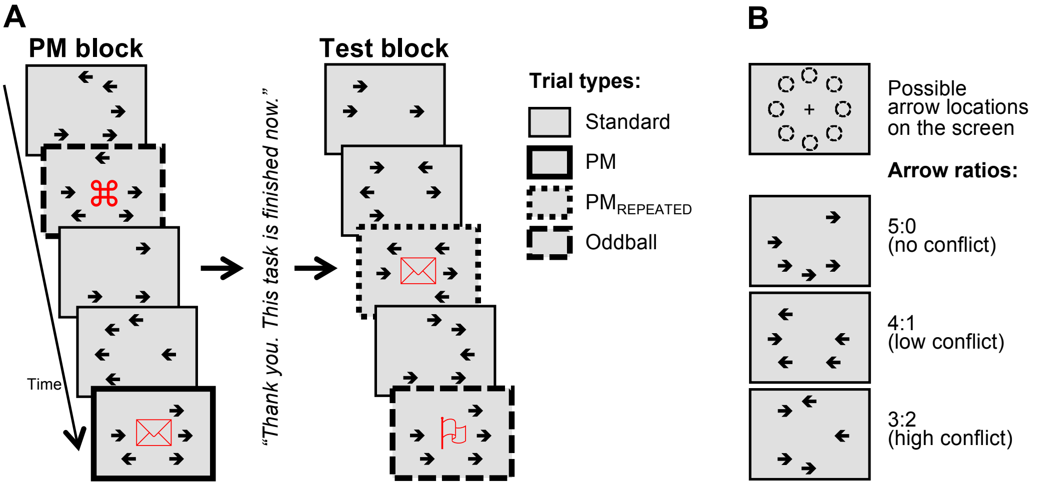

Figure 4. Procedure. (A) Illustration of trial sequences during PM and test blocks in the majority function task. Participants had to determine the direction of the majority of arrows in all ongoing-task trials except for prospective memory (PM) trials, in which they were required to press the spacebar instead. PM and PM$_{\text{REPEATED}}$ cues were red symbols that were presented in the center of the screen. Aftereffects of completed intentions were assessed during test blocks by comparing ongoing-task performance between PM$_{\text{REPEATED}}$ and oddball trials. (B) Illustration of possible arrow locations and arrow ratios in Experiment 1. In Experiment 2, arrows could appear at 12 possible locations and arrow ratios were 7:0, 6:1, 5:2, and 4:3.

3.4. Experiment 1

To assess the effects of cognitive-control availability and aging on intention deactivation, we compared aftereffects of completed intentions in PM$_{\text{REPEATED}}$ trials between arrow ratios 5:0 (no control demand), 4:1 (low demand), and 3:2 (high demand) in younger and older adults.

3.4.1. Methods

Ethics statement. The study was approved by the Institutional Review Board of the Technische Universität Dresden. All participants provided informed consent prior to performing any study procedures.

Participants. Twenty-four younger (18 female, 19–28 years, M_{age} = 24.1, SD_{age} = 2.2 years) and 24 older adults (13 female, 57–75 years, M_{age} = 68.3, SD_{age} = 5.7 years) took part in two sessions of the experiment lasting approximately 1.5 h each, in exchange for partial course credit or 15 €. Sum scores of the German *Mehrfachwortschatz Intelligenztest MWT-A* (English version: Multiple Choice Word Test-A; Lehrl, Merz, Burkhard, & Fischer, 1991) did not differ significantly between younger (M = 31.5, SD = 3.1) and older adults (M = 32.1, SD = 1.9), $t(37.94) = -0.84$, p = .407, $d = -0.24$, suggesting that age groups did not differ in premorbid intelligence.

Apparatus and Stimuli. The experiment was performed on a Windows XP SP2 personal computer running Presentation® software (Version 16.3, www.neurobs.com). A standard German (*QWERTZ*) keyboard was used to record participants' responses. Stimuli were presented in Wingdings font against a light gray background on a 19-inch monitor at a resolution of 1280 ×1024 pixels (viewing distance approx. 60 cm). For ongoing-tasks, participants were instructed to press the *X* key with their left index finger if the majority of arrows pointed towards the left and to press the *period* (.) key with their right index finger if the majority of arrows pointed to the right. For PM tasks, participants were instructed to interrupt the majority judgments and instead press the spacebar in response to a pre-specified symbol (PM cue; e.g., 🕭). Trials were composed of either three or five black arrows (font size 17 pt, visual angle ≈ 5.7°) that could appear at eight equidistant positions along the outline of an imaginary circle (radius ≈ 1.4 cm, vertical visual angle ≈ 32.8°; Figure 4B). The position of arrows was determined randomly for each trial. PM cues, $\text{PM}_{\text{REPEATED}}$ cues and oddballs comprised 30 symbols (e.g., ⌘, 🖂; font size 53 pt, vertical visual angle ≈ 17.7°, see Appendix A for all stimuli) that were presented in red at the center of the screen.

Procedure. At the beginning of each session, participants gave their informed consent and briefly practiced the ongoing task. Participants then performed eight cycles of a PM block that included an event-based PM task, and a subsequent ongoing-task-only test block. This design—two sessions with several PM-block–test-block cycles—allowed us to increase the number of aftereffect measurements while avoiding the individual session from being too long (Walser, Plessow, et al., 2014).

In order to facilitate PM performance and avoid a potential confound of aftereffect differences with differences in PM performance, instructions emphasized PM-task importance at the beginning of the experiment and at the start of each PM block (i.e., "*It is important that you concentrate on detecting the symbol* [PM cue].") (Walter & Meier, 2014). Instructions were displayed for 14 s at the beginning of a block. At the end of PM blocks, participants were informed that the current task was finished (i.e., "*Thank you. The current task is finished now.*", 3 s) and subsequently moved on to an ongoing-task-only test block.

Each PM and test block consisted of 72 trials (66 standard, 3 $\text{PM}/\text{PM}_{\text{REPEATED}}$, 3 oddball trials). Standard trials were 54 trials with five arrows (set size 5; 18 trials per arrow ratios 5:0, 4:1 and 3:2), and 12 trials with three arrows (set size 3; 6 trials per arrow ratios 3:0 and 2:1). We reasoned that including trials in set size 3 would reduce monotony during the experiment and could potentially increase general task-difficulty due to unexpected changes in arrow quantity in ongoing-task trials. PM or $\text{PM}_{\text{REPEATED}}$ and oddball cues were presented randomly after the first four trials of a block (standard trials only) and appeared only once during a block in each arrow ratio of set size 5 (5:0, 4:1, 3:2).

Trials started with the presentation of a black fixation cross (plus sign, Arial font, size 20 pt, vertical visual angle ≈ 6.7°) for 1000 ms, followed by the presentation of the imperative stimulus for a maximum of 5000 ms or until a response was given, followed by a blank screen for 400 ms. Errors and response omissions prompted a feedback tone (784 Hz) that was delivered through headphones for the first 200 ms of the blank screen. Deviant symbols for PM, $\text{PM}_{\text{REPEATED}}$ and oddball trials were drawn randomly from a pool of 30 symbols and were presented simultaneously with onset of ongoing-task stimuli.

3.4.2. Results

We conducted separate analyses for response times (RTs), commission errors and other error types (i.e., erroneous ongoing-task responses or response omissions) in trials of set-size 5 only, since only these contained PM and $\text{PM}_{\text{REPEATED}}$ stimuli. For RT analyses, errors (8.1% response failure or miss) and trials with RTs ± 2.5 *SD*s of a participant's mean RT for a given arrow ratio and trial type within each block were excluded (2.3%). The relevant data is set forth in Figure 5 and Table 2.

Manipulation checks. In order to test whether increasing cognitive-control demands decreased cognitive-control availability during task performance and how this was affected by aging, we conducted repeated-measures ANOVAs involving the factors age (younger vs. older adults) and arrow ratio (5:0, 4:1, 3:2) on ongoing-task standard-trial RTs and error rates (excluding erroneous PM responses[5]) that were averaged across PM and test blocks. Subsequently, to determine whether potential effects of cognitive-control demand and/or age on aftereffects of completed intentions could be the result of differences in PM performance, we conducted similar ANOVAs on PM-trial RTs and error rates.

Ongoing-task performance. Older adults responded slower in standard trials (1280 ms) than younger adults (803 ms), $F(1, 46) = 44.01$, $p < .001$, $\eta_p^2 = .49$. As indicated by a significant main effect of arrow ratio, $F(2, 92) = 251.06$, $p < .001$, $\eta_p^2 = .85$, increasing the arrow ratio resulted in longer ongoing-task RTs (5:0: 653 ms, 4:1: 1000 ms, 3:2: 1472 ms). A significant Arrow ratio × Age interaction, $F(2, 92) = 10.88$, $p = .002$, $\eta_p^2 = .19$, with a

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[5] We excluded erroneous PM responses from this manipulation check, since we conducted a separate analysis of commission-errors for the main analyses.

significant linear contrast, $F(1, 46) = 11.31$, $p = .002$, $\eta_p^2 = .20$, revealed that increasing the arrow ratio produced a stronger increase in ongoing-task RTs in older than in younger adults.

Error rates were not affected by age (younger adults: 8.3%, older adults: 7.2%), $F(1, 46) = 0.76$, $p = .387$, $\eta_p^2 = .02$. Similar to RT data, increasing the arrow ratio increased error rates, $F(2, 92) = 190.37$, $p < .001$, $\eta_p^2 = .81$ (5:0: 0.8%, 4:1: 3.6%, 3:2: 18.9%). Arrow ratio did not interact with age, $F(2, 92) = 0.73$, $p = .483$, $\eta_p^2 = .02$.

PM performance. Younger adults responded faster to PM cues (755 ms) than older-adults (1084 ms), $F(1, 46) = 41.46$, $p < .001$, $\eta_p^2 = .47$. We found a small effect of arrow ratio, $F(2, 92) = 5.65$, $p = .005$, $\eta_p^2 = .11$. Repeated contrasts revealed slower PM responses in 5:0 (940 ms) compared to 4:1 PM trials (896 ms), $F(1, 46) = 10.93$, $p = .002$, $\eta_p^2 = .19$, which were faster than PM responses in 3:2 PM trials (921 ms), $F(1, 46) = 4.10$, $p = .049$, $\eta_p^2 = .08$. This effect did not differ between age groups, $F(2, 92) = 0.56$, $p = .570$, $\eta_p^2 = .01$.

We found no effect of age on PM-error rates (younger adults: 12.3%; older adults: 14.0%), $F(1, 46) = .40$, $p = .530$, $\eta_p^2 = .01$. Surprisingly, PM-error rates declined with increasing arrow ratio, $F(2, 92) = 24.63$, $p < .001$, $\eta_p^2 = .35$. Repeated contrasts revealed that this effect was most likely driven by a strong decrease in error rates from 5:0 (19.7%) to 4:1 trials (9.1%), $F(1, 46) = 44.03$, $p < .001$, $\eta_p^2 = .49$, since arrow ratio did not affect error rates in 4:1 compared to 3:2 trials (10.7%), $F(1, 46) = 1.18$, $p = .283$, $\eta_p^2 = .03$. As indicated by a significant Arrow ratio × Age interaction, $F(2, 92) = 5.26$, $p = .008$, $\eta_p^2 = .10$, and planned contrasts, increasing the arrow ratio from 5:0 to 4:1 produced a stronger decrease in PM errors in the younger (14.2%) than in older adults (5.9%), $F(1, 46) = 8.22$, $p = .006$ $\eta_p^2 = .15$. The difference between 4:1 and 3:2 trials did not differ between groups (younger adults: −1.5%, older adults: −1.5%), $F(1, 46) = 0.00$, $p = 1.000$, $\eta_p^2 = .00$.

Aftereffects of completed intentions. In order to investigate the effects of age and cognitive-control demand on intention deactivation, we conducted Arrow ratio (5:0, 4:1, 3:2) × Age (younger vs. older adults) repeated-measures ANOVAs on aftereffects of completed intentions in terms of performance differences between $\text{PM}_{\text{REPEATED}}$ and oddball trials in RTs, error rates (erroneous ongoing-task responses and response omissions) and commission-error rates during test blocks (see Walser et al., 2017, for a similar analysis). Additionally, in line with previous research on commission errors (Anderson & Einstein, 2017; Bugg et al., 2016), we also analyzed the proportion of participants who made at least one commission error during test blocks. To determine statistical significance of the hypotheses-relevant effects, age effects on intention deactivation were tested at a Bonferroni-corrected alpha level of .0125 (.05/4), whereas cognitive-control demand effects and Arrow ratio × Age interactions were tested at an alpha level of .0167 (.05/3), respectively. Note that *p* values for tests at the Bonferroni-corrected alpha levels are denoted as p^*. Effect sizes for aftereffects of completed intentions and paired-samples *t*-tests refer to standardized within-subject mean differences (d_{rm}; Lakens, 2013).

RTs. Due to missing RT data in 43 out of 48 PM_{REPEATED} trials, one older adult was excluded from RT analysis. We found that, while older adults exhibited nominally larger aftereffects (98 ms, $d_{rm} = 0.21$) than younger adults (54 ms, $d_{rm} = 0.14$), this difference did not reach significance at the Bonferroni-corrected alpha level, $F(1, 45) = 4.15$, $p^* = .048$, $\eta_p^2 = .09$. Aftereffects were affected by arrow ratio, $F(2, 90) = 4.99$, $p^* = .014$, $\eta_p^2 = .10$. Planned comparisons showed that aftereffects in 5:0 trials (110 ms, $d_{rm} = 0.26$) were larger than in 4:1 trials (21 ms, $d_{rm} = 0.05$), $F(1, 45) = 11.72$, $p = .001$, $\eta_p^2 = .21$, which were smaller than aftereffects in 3:2 trials (95 ms, $d_{rm} = 0.18$), $F(1, 45) = 6.79$, $p = .012$, $\eta_p^2 = .004$. Since visual inspection of the data suggested that aftereffects might differ between the extreme poles of cognitive-control availability, we also conducted a post-hoc *t* test that revealed no difference in aftereffects between 5:0 and 3:2 trials, $t(46) = -0.37$, $p = .714$, $d_{rm} = -0.07$. The effect of arrow ratio on aftereffects did not differ between groups, $F(2, 90) = 0.38$, $p^* = .646$, $\eta_p^2 = .01$.

Errors. We found no significant effects on aftereffects in error rates, all Fs $\leqslant 1.20$, p^*s $\geqslant .29$, η_p^2s $\leqslant .01$.

Commission errors. Erroneous PM responses in test blocks occurred mostly in PM_{REPEATED} trials (younger adults: 4, older adults: 80), with few commission errors in oddball trials (younger adults: 1, older adults: 17) and four commission errors by older adults in standard trials (Table 3). The majority of commission errors in PM_{REPEATED} trials was made by two older adults in either the first (67% [16 out of 24 PM_{REPEATED} trials]) or both experimental sessions (90% [36 out of 48 PM_{REPEATED} trials]). While on a descriptive level, older adults produced larger commission-error aftereffects (5.47%, $d_{rm} = 0.31$) than younger adults (0.3%, $d_{rm} = 0.23$), this difference failed to reach significance at the Bonferroni-corrected alpha level, $F(1, 46) = 3.47$, $p^* = .069$, $\eta_p^2 = .07$. Commission-error aftereffects did not differ statistically between arrow ratios, $F(2, 92) = 1.52$, $p^* = .227$, $\eta_p^2 = .03$ (3.4%, $d_{rm} = 0.25$; 2.3%, $d_{rm} = 0.14$; 2.9%, $d_{rm} = 0.24$, in arrow rations 5:0, 4:1 and 3:2, respectively). Arrow ratio did not interact with age, $F(2, 92) = 1.14$, $p^* = .309$, $\eta_p^2 = .02$.

In line with previous studies (Anderson & Einstein, 2017; Scullin et al., 2012), we also analyzed the proportion of participants who made at least one commission error in PM_{REPEATED} or oddball trials during test blocks. The analysis revealed that, on a descriptive level, more older adults (n = 12) than younger adults (n = 4) made a commission error. This age effect, however, did not reach significance at the Bonferroni-corrected alpha level, $\chi^2(1) = 6.00$, $p^* = .014$, OR = 5.00.

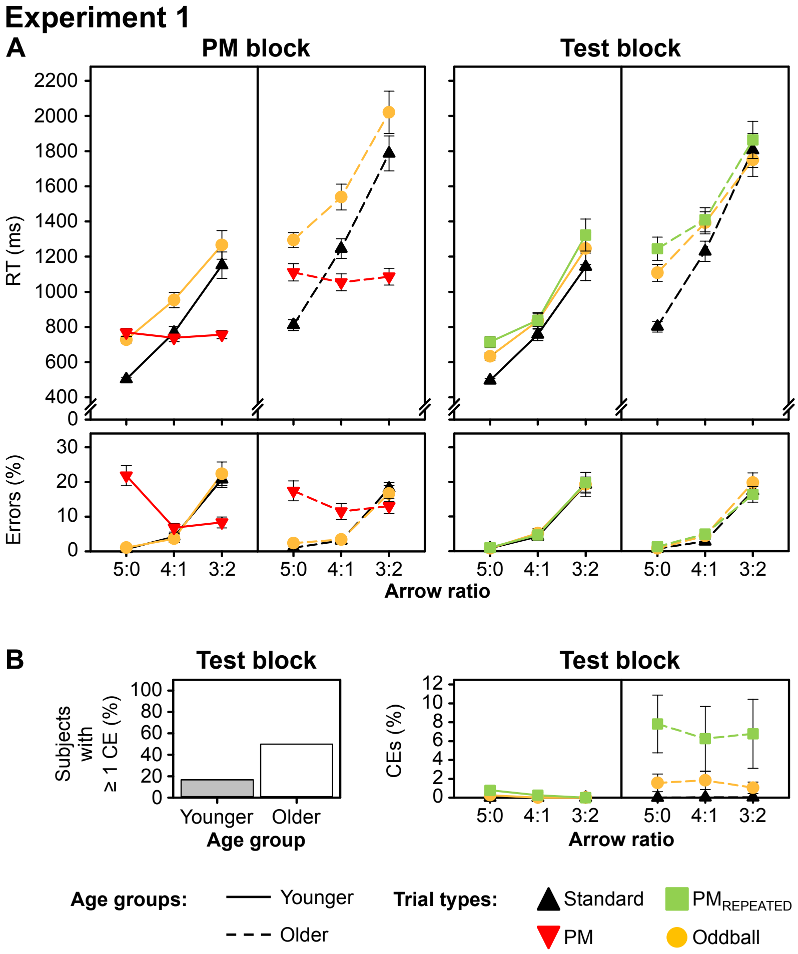

Figure 5. Results from Experiment 1. (A) Mean response times (RT), error rates (erroneous ongoing-task responses and response omissions) for performance in PM blocks and test blocks as a function of trial type (standard, PM, PM$_{\text{REPEATED}}$, oddball), arrow ratio (5:0, 4:1, 3:2) and age (younger adults, older adults). (B) Proportion of younger and older adults who made at least one commission error and mean commission-error (CE) rates during test blocks as a function of trial type, arrow ratio and age group. Error bars represent standard errors of the mean.

Table 2

Mean (SD) Performance during PM- and Test blocks in Experiment 1.

	Arrow ratio (cognitive-control demand)					
	5:0		**4:1**		**3:2**	
Trial type	**RT (ms)**	**Errors (%)**	**RT (ms)**	**Errors (%)**	**RT (ms)**	**Errors (%)**
Younger adults						
PM blocks						
Standard	503 (57)	0.6 (0.7)	768 (170)	4.2 (4.4)	1152 (368)	20.8 (11.6)
PM	769 (115)	21.9 (14.4)	739 (109)	6.8 (6.1)	756 (114)	8.3 (7.7)
Oddball	728 (92)	1.0 (4.0)	953 (211)	3.6 (5.5)	1266 (399)	22.4 (16.6)
Test blocks						
Standard	496 (58)	0.9 (1.4)	757 (172)	4.2 (4.6)	1142 (385)	19.3 (10.6)
PMREPEATED	715 (155)	1.0 (2.4)	838 (180)	4.7 (6.5)	1323 (447)	19.8 (14.7)
Oddball	633 (106)	0.8 (2.1)	835 (227)	5.2 (6.3)	1246 (446)	19.3 (16.7)
Aftereffects[a]	82 (72)	0.3 (2.2)	4 (117)	−0.5 (5.2)	77 (228)	0.5 (12.8)
Older adults						
PM blocks						
Standard	811 (151)	1.0 (2.2)	1246 (273)	3.1 (3.0)	1787 (487)	18.1 (8.5)
PM	1111 (238)	17.4 (14.0)	1054 (236)	11.5 (11.3)	1086 (231)	13.0 (10.6)
Oddball	1295 (204)	2.3 (4.0)	1539 (361)	3.4 (4.1)	2020 (591)	16.7 (11.5)
Test blocks						
Standard	802 (151)	0.7 (1.5)	1230 (282)	2.8 (2.5)	1805 (474)	17.3 (8.2)
PMREPEATED	1245 (324)	1.3 (4.1)	1409 (332)	4.9 (5.8)	1864 (518)	16.4 (11.0)
Oddball	1108 (236)	0.8 (2.8)	1392 (310)	4.4 (5.4)	1750 (459)	19.8 (13.8)
Aftereffects[a]	137 (137)	0.5 (4.8)	40 (134)	0.5 (7.1)	114 (405)	−3.4 (13.5)

Note. Error rates represent erroneous ongoing-task responses and response omissions.

[a]Aftereffects represent performance differences between PMREPEATED and oddball trials.

Table 3

Mean (SD) Commission-error (CE) rates during Test Blocks in Experiment 1.

	Arrow ratio (cognitive-control demand)		
	5:0	**4:1**	**3:2**
Trial type	**CE (%)**	**CE (%)**	**CE (%)**
Younger adults			
Test block			
Standard	-	-	-
PMREPEATED	0.8 (2.1)	0.3 (1.3)	-
Oddball	0.3 (1.3)	-	-
Aftereffects[a]	0.5 (2.6)	0.3 (1.3)	-
Older adults			
Test block			
Standard	-	0.0 (0.1)	0.0 (0.1)
PMREPEATED	7.8 (15.0)	6.2 (16.8)	6.8 (18.0)
Oddball	1.6 (4.6)	1.8 (4.7)	1.0 (3.0)
Aftereffects[a]	6.2 (11.5)	4.4 (13.1)	5.7 (16.9)

[a]Aftereffects represent performance differences between PMREPEATED and oddball trials.

3.4.3. Discussion

In Experiment 1, we compared effects of trial-by-trial cognitive-control availability on intention deactivation between younger and older adults. Regarding our manipulation of cognitive-control demands, results were straight-forward: Increasing the arrow ratio in ongoing-task trials produced the expected decline in ongoing-task performance (see also Fan, 2014; Fan et al., 2008), indicating that increasing cognitive-control demands seems to have decreased cognitive-control availability during task performance. This demand-dependent response slowing in standard trials was more pronounced in older adults, which was not mirrored by error data. Such results might be indicative of a greater susceptibility of older adults for transient cognitive-control-demand effects on response initiation and/or a compensatory performance slowing of elderly to maintain high quality of ongoing-task performance (e.g., Kopp, Lange, Howe, & Wessel, 2014).

As expected, PM performance with salient PM cues was high and PM error rates did not increase as a function of cognitive-control demand. By contrast, we even found an unexpected decrease in PM error rates with increasing cognitive-control demand. As our analysis of ongoing-task performance suggests, however, this was likely the result of overall faster responses during trials in arrow ratio 5:0 leading participants to miss PM cues more often than in other arrow ratios. This overlooking of PM cues in 5:0 trials seems to have been exacerbated by overall faster ongoing-task and PM responses in younger adults, which led to a slightly more pronounced PM-error decrease with increasing arrow ratio in younger compared to older adults.

Older adults exhibited slowed PM performance while PM accuracy was not affected by age. This mirrors findings of comparable accuracy but slowed performance in older compared to younger adults in task switching (Kopp et al., 2014) and provides further evidence for spontaneous PM-retrieval processes with salient PM cues being spared with aging (Bugg et al., 2016; Kretschmer-Trendowicz & Altgassen, 2016; Scullin et al., 2011). The latter finding also renders it unlikely that potential age effects on intention deactivation could be confounded by age-related PM-performance differences.

Regarding age effects on intention deactivation, we replicated previous findings of nominally increased aftereffects of completed intentions in terms of performance costs in $\text{PM}_{\text{REPEATED}}$ compared to oddball trials as well as nominally increased commission-error aftereffects and a tendency for an increased number of participants who made a commission error in the older-adults compared to the younger-adults group (Scullin & Bugg, 2013; Scullin et al., 2012). Moreover, contrary to our hypothesis, effects of cognitive-control demand on intention deactivation did not differ between younger and older adults. This was especially surprising, given that we found stronger cognitive-control-demand effects on ongoing-task performance in older compared to younger adults. We will discuss potential mechanisms of this age effect on intention deactivation in the general discussion.

Most importantly, regarding the modulation of intention deactivation by transient cognitive-control availability, results were ambiguous. We found no modulation of aftereffects in error or commission-error rates by cognitive-control demands. Similarly, RT aftereffects did not differ between extreme poles of control demands (5:0 vs. 3:2 trials), suggesting that intention deactivation does not depend on trial-by-trial cognitive-control availability. This interpretation is challenged strongly, however, by the absence of aftereffects during low-conflict trials (4:1). As an alternative, one could argue that due to increased processing demands of conflict compared to no-conflict trials (e.g., Botvinick et al., 2001; Braver, 2012), our manipulation of cognitive-control availability might affect intention deactivation only during conflict trials. That is, while low-conflict trials might have attenuated processing of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues via increased shielding of the ongoing task (Goschke & Dreisbach, 2008), high-conflict trials might have impaired focusing attention on ongoing-task stimuli (e.g., Lavie, 2005) and consequently exacerbated interference from $\mathrm{PM}_{\mathrm{REPEATED}}$ cues.

3.5. Experiment 2

We conducted Experiment 2 to test if we could replicate the absence of aftereffects during low-conflict trials from Experiment 1, and to allow a more fine-grained investigation (no conflict vs. low conflict vs. medium conflict vs. high conflict) of the potential modulation of intention deactivation by transient cognitive-control demands during conflict trials. We reasoned that if intention deactivation was modulated by cognitive-control availability only during conflict trials, then aftereffects should be decreased in low-, compared to no-conflict trials, but should increase linearly with increasing conflict strength across medium- and high-conflict trials. Since cognitive-control-demand effects on intention deactivation did not differ between younger and older adults in Experiment 1, we only investigated younger adults in Experiment 2.

3.5.1. Methods

Participants. Forty-eight participants (35 female, 19–29 years, M_{age} = 23.8, SD_{age} = 2.9 years) took part in two sessions lasting approximately 1.5 h each, in exchange for partial course credit or 15 €.

Stimuli and Procedure. Stimuli and procedure for Experiment 2 were similar to Experiment 1, with the following changes: Ongoing-task trials comprised five or seven arrows that could appear at 12 possible locations on the screen. PM and test blocks consisted of 98 trials (90 standard, 4 PM/$PM_{REPEATED}$, 4 oddball trials), with 80 trials in set size 7 (20 trials per arrow ratios 7:0, 6:1, 5:2 and 4:3) and 18 trials in set size 5 (6 trials per arrow ratios 5:0, 4:1 and 3:2). Deviant symbols for PM, $PM_{REPEATED}$ and oddball trials were drawn randomly from a pool of 32 symbols. Note that PM or $PM_{REPEATED}$ symbols were presented pseudo-randomly throughout a block, with the restriction, that at least five trials were in between PM or $PM_{REPEATED}$ trial presentations.

3.5.2. Results

Analyses were conducted on trials of set-size 7 only, since only these contained PM and $PM_{REPEATED}$ stimuli. For RT analyses, errors (10.8% response failure or miss) and trials with RTs ± 2.5 SDs of a participant's mean RT for a given arrow ratio and trial type within each block were excluded (2.5%). The relevant data is set forth in Figure 6 and Table 4

Manipulation checks. Similar to Experiment 1, we first examined the effects of cognitive-control demands on ongoing-task performance to assess whether increasing control demands decreased cognitive-control availability. To this aim, we conducted repeated-measures ANOVAs on standard-trial RTs and error rates (excluding erroneous PM responses[5]) averaged across PM and test blocks as a function of arrow ratio (7:0, 6:1, 5:2, 4:3). Subsequently, we analyzed PM performance (RTs, error rates) in similar ANOVAS to determine whether potential cognitive-control-demand effects on aftereffects of completed intentions could be the result of PM-performance differences between cognitive-control demands.

Ongoing-task performance. Standard-trial RTs increased with increasing arrow ratio, $F(3, 141) = 93.82$, $p < .001$, $\eta_p^2 = .67$, (7:0: 530 ms, 6:1: 670 ms, 5:2: 878 ms, 4:3: 1177 ms). This effect was mirrored by error data, $F(3, 141) = 565.02$, $p < .001$, $\eta_p^2 = .92$, (7:0: 1.2%, 6:1: 3.1%, 5:2: 9.9%, 4:3: 28.1%).

PM performance. Arrow ratio did not affect RTs in PM trials (Table 4), *F*(3, 141) = 1.69, *p* = .173, η_p^2 = .04. PM-error rates decreased with increasing arrow ratio, *F*(3, 141) = 16.11, *p* < .001, η_p^2 = .26, in a linear fashion from 7:0 PM trials (22.3%) to 6:1 (14.7%) and 5:2 trials (10.8%), with a nominal increase to 4:3 PM trials (12.6%), *F*(1, 47) = 33.56, *p* < .001, η_p^2 = .42 (linear contrast).

Aftereffects of completed intentions. To examine cognitive-control-demand effects on intention deactivation, we conducted repeated-measures ANOVAs involving the factor arrow ratio (7:0, 6:1, 5:2, 4:3) on aftereffects of completed intentions (i.e., performance differences between $\mathrm{PM}_{\mathrm{REPEATED}}$ and oddball trials) in RTs, commission errors and other error types (i.e., erroneous ongoing-task responses or response omissions). To determine statistical significance of the hypotheses-relevant effects of cognitive-control demands on intention deactivation, we used a Bonferroni-corrected alpha level of .0167 (.05/3). Note that *p* values for tests at Bonferroni-corrected alpha levels are denoted as *p**.

RTs. Aftereffects of completed intentions did not differ statistically between arrow ratios, *F*(3, 141) = 0.62, *p** = .549, η_p^2 = .01 (69 ms, d_{rm} = 0.35; 73 ms, d_{rm} = 0.30; 98 ms, d_{rm} = 0.26; 49 ms, d_{rm} = 0.08 in arrow ratios 7:0, 6:1, 5:2, 4:3, respectively). Testing whether aftereffects differed between extreme poles of cognitive-control availability, as was suggested by visual inspection of the data, revealed no statistically significant difference between arrow ratio 7:0 and 4:3, *t*(47) = 0.52, *p* = .608, d_{rm} = 0.09.

Errors. Similar to RT data, we found no effect of arrow ratio on aftereffects of completed intentions in error rates, *F*(3, 141) = 0.14, *p** = .839, η_p^2 = .003.

Commission errors. Commission errors were rare and occurred almost exclusively in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials (0.9% [27 trials]); only two commission errors occurred in standard trials (Table 5). Commission-error aftereffects were affected by arrow ratio, *F*(3, 141) = 4.50, *p** = .010, η_p^2 = .09 (1.8%, d_{rm} = 0.82; 0.9%, d_{rm} = 0.58; 0.3%, d_{rm} = 0.29; 0.5%, d_{rm} = 0.42 in arrow ratios 7:0, 6:1, 5:2, 4:3, respectively). Repeated contrasts revealed only nominally decreased commission-error aftereffects from 7:0 to 6:1 trials, *F*(1, 47) = 3.00, *p* = .090, η_p^2 = .06, and from 6:1 to 5:2 trials, *F*(1, 47) = 3.78, *p* = .058, η_p^2 = .07. Commission-error aftereffects also did not differ significantly between 5:2 and 4:3 trials, *F*(1, 47) = 0.66, *p* = .420, η_p^2 = .01. Additionally, testing the difference in commission-error aftereffects between the extreme poles of cognitive-control availability in this study (7:0 vs. 4:3), revealed larger aftereffects in 7:0 than in 4:3 trials, *t*(47) = 2.34, *p* = .024, d_{rm} = 0.52.

3.5.3. Discussion

In Experiment 2, we investigated effects of trial-by-trial cognitive-control demands on intention deactivation in younger adults. Results were straightforward: First, similar to Experiment 1, increasing the arrow ratio decreased speed and accuracy of ongoing-task performance; possibly reflecting a demand-dependent decrease in cognitive-control availability. Second, we again found decreasing PM-error rates with increasing cognitive-control demands, which may result from participants missing PM cues more often during trials in arrow ratios 7:0 and 6:1 due to their overall faster ongoing-task performance in these trials. Most importantly and in contrast to Experiment 1, we found no evidence for a modulation of aftereffects of completed intentions in terms of performance costs in PM$_{\text{REPEATED}}$ compared to oddball trials by cognitive-control availability. That is, RT and error aftereffects were not modulated by transient cognitive-control demands in PM$_{\text{REPEATED}}$ trials. Interestingly, however, we found that commission-error aftereffects decreased with increasing cognitive-control demands. We will discuss these effects in more detail in the general discussion.

Table 4

Mean (SD) Performance during PM- and Test Blocks in Experiment 2.

	Arrow ratio (cognitive-control demand)							
	7:0		**6:1**		**5:2**		**4:3**	
Trial type	**RT (ms)**	**Errors (%)**	**RT (ms)**	**Errors (%)**	**RT (ms)**	**Errors (%)**	**RT (ms)**	**Errors (%)**
PM blocks								
Standard	532 (132)	1.1 (2.2)	676 (211)	2.9 (3.9)	883 (322)	9.7 (7.6)	1190 (542)	28.1 (8.3)
PM	797 (150)	22.3 (14.3)	795 (162)	14.7 (11.6)	810 (155)	10.8 (12)	787 (138)	12.6 (12.7)
Oddball	730 (186)	2.5 (5.1)	840 (262)	4.4 (6.7)	1053 (373)	12.4 (9.9)	1370 (602)	28.8 (12.3)
Test blocks								
Standard	527 (122)	1.4 (2.9)	663 (189)	3.3 (4.2)	874 (297)	10.2 (7.8)	1165 (515)	28.1 (8.0)
PM$_{\text{REPEATED}}$	721 (200)	1.6 (3.8)	806 (253)	4.4 (6.9)	1033 (368)	9.2 (11)	1336 (622)	30.2 (14)
Oddball	652 (179)	2.3 (6)	733 (222)	4.3 (6.6)	935 (377)	10.3 (9.5)	1287 (561)	31.1 (13.1)
Aftereffects[a]	69 (72)	−0.8 (4.8)	73 (118)	0.1 (5.1)	98 (210)	−1.0 (8.3)	49 (273)	−0.9 (16.5)

Note. Error rates represent erroneous ongoing-task responses and response omissions.

[a] Aftereffects represent performance differences between PM$_{\text{REPEATED}}$ and oddball trials.

Table 5

Mean (SD) Commission-error (CE) Rates during Test Blocks in Experiment 2.

	Arrow ratio (cognitive-control demand)			
	7:0	**6:1**	**5:2**	**4:3**
Trial type	**CE (%)**	**CE (%)**	**CE (%)**	**CE (%)**
Test blocks				
Standard	-	-	0.0 (0.1)	0.0 (0.1)
PM$_{\text{REPEATED}}$	1.8 (3.1)	0.9 (2.2)	0.3 (1.3)	0.5 (1.7)
Oddball	-	-	-	-
Aftereffects[a]	1.8 (3.1)	0.9 (2.2)	0.3 (1.3)	0.5 (1.7)

[a] Aftereffects represent performance differences between PM$_{\text{REPEATED}}$ and oddball trials.

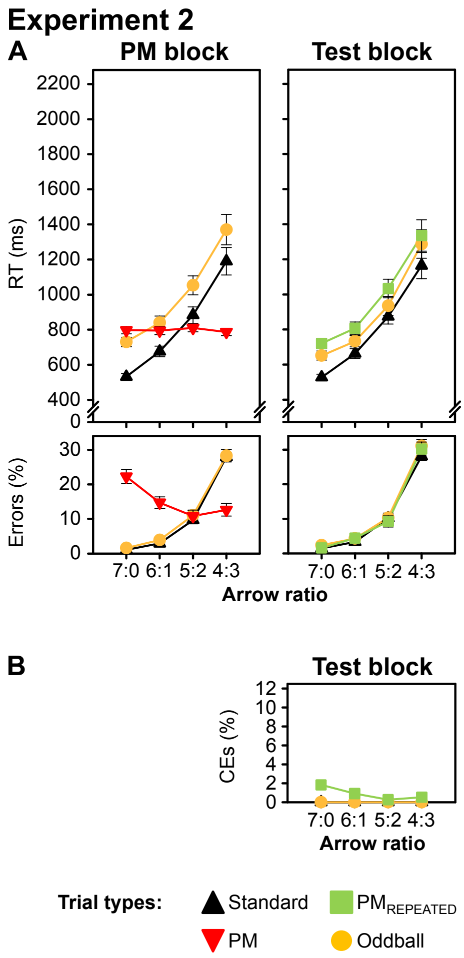

Figure 6. Results from Experiment 2. (A) Mean response times (RT) and error rates (erroneous ongoing-task responses and response omissions) for performance in PM blocks and test blocks in Experiment 2 as a function of trial type (standard, PM, PM$_{\text{REPEATED}}$, oddball) and arrow ratio (7:0, 6:1, 5:2, 4:3). (B) Mean commission-error (CE) rates during test blocks as a function of trial type and arrow ratio. Error bars represent standard errors of the mean.

3.6. General Discussion

The aim of the present study was to investigate the effects of aging and transient availability of cognitive-control resources during encounters of no-longer-relevant PM cues on intention deactivation. We assessed aftereffects of completed intentions under varying degrees of cognitive-control demands in younger and older adults (Experiment 1) and with a more fine-grained parametric variation of cognitive-control demands during PM$_{\text{REPEATED}}$ trials (Experiment 2). In line with the dual mechanisms account of PM commission errors (Bugg et al., 2016; Scullin & Bugg, 2013; see also Meiser & Rummel, 2012), we expected that if transient cognitive-control availability during encounters of PM$_{\text{REPEATED}}$ cues was crucial for intention deactivation, aftereffects of completed intentions should increase with increasing cognitive-control demands on ongoing-task processing in PM$_{\text{REPEATED}}$ trials.

Moreover, based on previous findings (e.g., Scullin et al., 2012), we expected that aging should increase aftereffects of completed intentions and exacerbate potential effects of cognitive-control demands on aftereffects of completed intentions.

First, we found evidence for a successful reduction of cognitive-control availability by increasing demands on cognitive control. That is, in both experiments we observed the characteristic increase in ongoing-task RTs and error rates with increasing trial-by-trial cognitive-control demands in a majority-function task (Fan, 2014; Fan et al., 2008). Second, in line with previous findings (e.g., Scullin & Bugg, 2013; Scullin et al., 2012), older adults exhibited a nominally higher risk of making a commission error after intention completion than younger adults. Note that these age effects could not be explained by impaired PM performance in older adults, as both groups did not differ in that regard.

Third and most importantly, transient cognitive-control availability did not consistently modulate intention deactivation: In Experiment 1, RT and error aftereffects of completed intentions did not differ between extreme-poles of cognitive-control availability (i.e., between no-conflict and high-conflict trials) in both younger and older adults. Surprisingly, taxing cognitive control only a little (during low-conflict trials), led to an almost complete absence of RT aftereffects. These initial findings suggested that intention deactivation is either not modulated by transient cognitive-control availability during $\mathrm{PM}_{\text{REPEATED}}$-cue encounters or only modulated by cognitive-control availability during trials that place an additional burden on cognitive control, for instance, by inducing additional conflicts in stimulus processing. Astonishingly, however, even with an extended manipulation of cognitive-control demands in Experiment 2 (i.e., no conflict vs. low conflict vs. medium conflict vs. high conflict), RT and error aftereffects of completed intentions were not affected by transient cognitive-control demands. Surprisingly, we found that commission-error rates in Experiment 2 decreased with increasing cognitive-control demands. While it may be tempting to explain this effect with an attenuated processing of distractor stimuli during conflict trials, as has been observed in conflict-adaptation studies (e.g., Wendt et al., 2014), note that the overall number of commission errors in Experiment 2 was very low and that we did not observe this effect in Experiment 1. It is also important to note that commission errors in PM are notoriously difficult to assess. Even under optimal conditions that foster spontaneous intention retrieval—i.e., using salient focal PM cues, matching ongoing tasks, few PM cue representations (McDaniel et al., 2015)—commission errors after intention completion can be quite rare (Anderson & Einstein, 2017). Although there are task conditions that seemingly produce higher rates of erroneous PM responses than using completed intentions, for instance, using active intentions (Boywitt et al., 2015), instructions to suppress PM responses (Meiser & Rummel, 2012), cancelling PM-task performance or providing no opportunities to perform the PM task (Bugg & Scullin, 2013), these manipulations necessarily also shift the topic of investigation away from aftereffects of completed intentions. Thus, additional research is required to determine if and under which conditions transient cognitive-control demands affect commission-error rates or other measures of intention deactivation after intention completion.

Based on the present findings, we suggest that while aging may increase aftereffects of completed intentions, intention deactivation might not be affected by transient cognitive-control availability during encounters of no-longer-relevant PM cues. Although our findings dovetail with previous reports of age-related declines in intention deactivation (e.g., Bugg et al., 2016) and corroborate the notion that intention deactivation does not rely on available cognitive resources after intention completion (Walser, Goschke, et al., 2014), it remains an open question how age effects on intention deactivation arise and through which mechanisms completed intentions are deactivated.

3.6.1. Effects of Age on Intention Deactivation

Previous reports of increased aftereffects of completed intentions or more frequent commission errors in older adults with completed (Scullin et al., 2012) or suspended intentions (Boywitt et al., 2015) have been attributed to a supposed decline in cognitive-control abilities with cognitive aging (Hasher, Stoltzfus, Zacks, & Rypma, 1991). However, the notion of a general age-related cognitive-control impairment is not undisputed (e.g., Cona, Arcara, Amodio, Schiff, & Bisiacchi, 2013; Lustig & Jantz, 2015; Verhaeghen, 2011). Importantly, given that in the present study aging only modulated cognitive-control-demand effects on ongoing-task performance but not on aftereffects of completed intentions. In light of these selective effects of aging, older adults' impaired intention deactivation cannot be accounted for by an overall impairment of cognitive-control processes. Rather, our findings highlight the necessity to determine which specific cognitive-control functions are affected by aging to elucidate the mechanisms for age effects in PM and intention deactivation. Although, based on the present study, we cannot rule out that age effects on intention deactivation arise due to deficits in response inhibition, there are several alternative mechanisms through which impaired intention deactivation in older adults could arise, that we believe would be useful for future research.

First, deficits in cognitive flexibility or shifting abilities could account for increased aftereffects in older adults. That is, several studies reported that older adults show impaired shifting abilities in terms of attenuated associations between a cue and a context (e.g., PM cue and PM block) (Lustig & Jantz, 2015), difficulties in activating and updating context information (Braver, Satpute, Rush, Racine, & Barch, 2005) or in disengaging from no-longer-relevant task sets when performing a single task after a period of task switching (i.e., fade-out costs; Mayr & Liebscher, 2001). Applying this logic to the present paradigm, we speculate that the lack of a strong association between the PM cue and the PM block in older adults could have led to increased interference from $\text{PM}_{\text{REPEATED}}$ cues in test blocks.

Second, increased aftereffects could also stem from older adults' difficulty in ignoring distracting information (Weeks & Hasher, 2014) that seems to increase with age (Giesen, Eberhard, & Rothermund, 2015). More specifically, difficulties in ignoring $\text{PM}_{\text{REPEATED}}$ cues could increase the risk of accidentally retrieving the completed PM-task set and/or the intended action, which would exacerbate aftereffects of completed intentions.

Lastly, age effects on cognitive control abilities dissociate between a proactive and a reactive mode of control (Manard, François, Phillips, Salmon, & Collette, 2017). Specifically, while older adults exhibit deficits in proactive control in terms of utilizing task information to anticipate conflicts during task performance (Braver et al., 2005; Manard, Carabin, Jaspar, & Collette, 2014), reactive control in terms of the ability to establish, maintain and quickly adapt to novel cue–attention associations is preserved (e.g., Bugg, 2014; Manard et al., 2017). Interestingly, older adults' deficit in proactive control in some cases is compensated by stronger mobilization of reactive control to uphold performance (Kopp et al., 2014). In the present study, high demands on proactive-control (i.e., due to mostly incongruent ongoing-task trials) might have led to a compensatory recruitment of reactive control in older adults. Consequently, reactive control processes required for intention deactivation would have been reduced and thereby led to an increase in aftereffects. It is unclear, however, whether this logic could also apply to previous studies on intention deactivation in older adults in which ongoing-task items were balanced for congruency (Bugg et al., 2016; Scullin & Bugg, 2013; Scullin et al., 2012, 2011). It seems at least feasible that older adults' compensatory recruitment of reactive control for ongoing-task performance could explain impairments in reactive control over intention deactivation that may ultimately lead to increased aftereffects of completed intentions.

3.6.2. Effects of Cognitive-Control Availability on Intention Deactivation

Our findings raise a number of questions regarding the mechanisms of intention deactivation: First, the surprising robustness of RT aftereffects of completed intentions even under extreme cognitive-control demands raises the question whether RT aftereffects can at all be modulated by top-down adjustments of cognitive control. The extent to which this might be possible could likely depend on the degree to which task features foster spontaneous intention retrieval. That is, when reliance on spontaneous retrieval is high (e.g., with salient PM cues), spontaneous intention retrieval might overshadow potential effects of cognitive-control demands on intention deactivation. Testing this, however, is difficult since reducing the likelihood of spontaneous retrieval also reduces the likelihood of observing aftereffects of completed intentions.

Second, the lack of demand-dependent effects on RT aftereffects and our observation of decreasing commission-error rates with increasing cognitive-control demands in the present study seems to be at odds with the dual mechanisms account of PM commission errors and previous reports of control-dependent modulations of aftereffects of completed intentions (Bugg et al., 2016; Scullin & Bugg, 2013; see also Meiser & Rummel, 2012). Note, however, that in the present study we focused on cognitive control in terms of uncertainty reduction. That is, our control-demand manipulation targeted the number of computations that are required for response selection in a trial (Fan, 2014; Wang, Liu, & Fan, 2011). Although shared-capacity models of cognitive control (e.g., Fan, 2014) propose that any type of cognitive load or mental operation should usurp cognitive resources that, according the dual-mechanisms account of PM commission errors, might be needed for successful intention deactivation, we found no evidence for this here. Hence, it remains an open question whether intention deactivation and ongoing-task performance after intention completion do rely on shared capacities and/or parallel processing of ongoing tasks and intention-related information. This seems at least feasible, given that the effects of cognitive-control demands in ongoing-task trials and effects of deviant stimuli on task performance in the present study seem to be additive.

Lastly, we would like to point out that a reduction of cognitive-control resources for intention deactivation due to increased ongoing-task shielding during conflict trials

(Goschke & Dreisbach, 2008) may not be the only mechanism through which transient cognitive-control demands could affect intention deactivation. Research on conflict-adaptation effects showed that prior experience of conflict can lead to more controlled task processing in terms of increased attention allocation towards target stimuli and decreased interference from distractor stimuli (e.g., Notebaert & Verguts, 2006; Wendt et al., 2014). If this cognitive-control adjustment happened online within a trial, increasing conflict strength in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials should actually reduce aftereffects of completed intentions. While we did not find conclusive evidence for this here, it is important to note that both mechanisms—(a) reduced cognitive-control resources for intention deactivation and (b) increased attention allocation towards ongoing-task stimuli—might not necessarily be mutually exclusive. It is feasible that, if both processes were affected by manipulations of cognitive-control demands in equal amounts, their effects on task performance would counteract each other, which could also explain the lack of cognitive-control-demand effects on intention deactivation in the present study. Importantly, however, future studies are required to determine if and under which conditions this is true and what the individual contribution of each process to a cognitive-control dependence of intention deactivation might be.

3.6.3. Conclusion

The present study was the first to investigate the effects of transient availability of cognitive control on intention deactivation in a task that required cognitive control in terms of perceptual grouping attempts for response selection during encounters of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues. Although this is arguably a critical time-point for intention deactivation, we did not find evidence for a cognitive-control dependence of intention deactivation in terms of RT and error aftereffects of completed intentions. However, we found that older adults tended to be at a higher risk to make a commission error after intention completion. Additionally, we found preliminary evidence for a cognitive-control dependence of commission-error occurrences. Clearly, future research is needed to ascertain (a) whether intention deactivation is modulated by other transient cognitive-control demands, (b) which markers of intention deactivation are affected by these demands, (c) during which phases after intention completion cognitive-control availability is most critical, and relatedly, (d) which factors differentiate situations or persons that show aftereffects of completed intentions and commission errors from those who do not?

Answering these questions would, for instance, require to test whether and how aftereffects as well as the processes underlying prospective remembering are modulated by factors that are known to alter cognitive-control functioning. One of these factors is acute stress. In brief, experiences of acute stress have been theorized to impair higher-order cognitive functioning related to the prefrontal cortex or the hippocampus (e.g., Arnsten, 2015) and have repeatedly been shown to modulate cognitive-control functions like response inhibition, working memory and selective attention (for an overview, see Shields et al., 2016). At the same time, associations of PM and intention deactivation to these cognitive-control functions (e.g., Bugg et al., 2016; Cona et al., 2015) and to stress-sensitive brain areas (e.g., Beck et al., 2014; Carlesimo et al., 2014) provide potential pathways through which acute stress could modulate these capacities. Therefore, the following two studies tested the effects of acute stress experiences after exposure to standardized stress-induction protocols on PM and intention deactivation. Additionally, in order to elucidate potential boundary conditions of such effects, these studies employed conditions that additionally posed varying demands on PM-cue detection, PM monitoring and intention deactivation (Study 2) as well as on ongoing-task performance (Study 3) that themselves have been shown to modulate PM (e.g., McDaniel et al., 2015; West & Bowry, 2005) and intention deactivation (e.g., Scullin et al., 2012).

4. Study 2 – Acute Stress Shifts the Balance Between Controlled and Automatic Processes in Prospective Memory

4.1. Abstract

In everyday life we frequently rely on our abilities to postpone intentions until later occasions (prospective memory; PM) and to deactivate completed intentions even in stressful situations. Yet, little is known about the effects of acute stress on these abilities. In the present work we investigated the impact of acute stress on PM functioning under high task demands. 1) Different from previous studies, in which intention deactivation required mostly low processing demands, we used salient focal PM cues to induce high processing demands during intention-deactivation phases. 2) We systematically manipulated PM-monitoring demands in a nonfocal PM task that required participants to monitor for either one or six specific syllables that could occur in ongoing-task words. Eighty participants underwent the Trier Social Stress Test, a standardized stress induction protocol, or a standardized control situation, before performing a computerized PM task. My primary interests were whether PM performance, PM-monitoring costs, aftereffects of completed intentions and/or commission-error risk would differ between stressed and non-stressed individuals and whether these effects would differ under varying task demands.

Results revealed that PM performance and aftereffects of completed intentions during subsequent performance were not affected by acute stress induction, replicating previous findings. Under high demands on intention deactivation (focal condition), however, acute stress produced a nominal increase in erroneous PM responses after intention completion (commission errors). Most importantly, under high demands on PM monitoring (nonfocal condition), acute stress led to a substantial reduction in PM-monitoring costs. These findings support ideas of selective and demand-dependent effects of acute stress on cognitive functioning. Under high task demands, acute stress might induce a shift in processing strategy towards resource-saving behavior, which seems to increase the efficiency of PM performance (reduced monitoring costs), but might increase initial susceptibility to automatic response activation after intention completion.

4.2. Introduction

Prospective memory (PM) is an essential human capacity that enables goal-directed behavior. It describes the ability to form and maintain an intention in memory and to postpone its execution until the encounter of a certain occasion (event-based PM) or time point (time-based PM) in the future (e.g., McDaniel & Einstein, 2007). This ability to disengage intended actions from the immediate present for future execution is related to higher-order cognitive functions that form the basis of complex behavior, planning, and anticipation of future action consequences (e.g., Goschke, 2013; Miyake et al., 2000). Everyday examples include taking medication at a pre-specified time schedule or congratulating a colleague to her birthday upon encounter of the colleague during a day at the office. These examples demonstrate that we often need to maintain intentions while engaging in other activities (i.e., ongoing tasks). They also illustrate that PM entails a number of dissociable components such as the active monitoring for events (PM cues) that indicate the retrieval of the intention (e.g., McDaniel & Einstein, 2000; R. E. Smith, 2003), the actual retrieval and execution of the intention (e.g., Ellis, 1996), and the deactivation of the intention representation after intention execution (e.g., A.-L. Cohen et al., 2017; Scullin et al., 2012; Walser et al., 2012, 2017).

Given the importance of PM functioning in everyday life (Kliegel & Martin, 2003), it is essential to understand the reliability and potential limits of successful PM functioning in extreme everyday situations, such as being exposed to an acute stressor. In the present study, we aimed at testing the reliability of different PM components following an acute psychosocial stress experience.

Stress is an important influence on modern private and work life (e.g., Kalia, 2002). Increasing work intensity and complexity that are related to multiple task performance and increased numbers of task interruptions (European Foundation for the Improvement of Living and Working Conditions, 2015; Lohmann-Haislah, 2012) not only place high demands on PM (Dodhia & Dismukes, 2009) and can lead to severe PM failures (Dismukes & Nowinski, 2007; Loft, 2014) but have also been associated with elevated levels of experienced stress (S. Cohen et al., 2007). The link between an acute stress experience and PM performance, however, is not well understood to date.

The experience of acute stress not only affects subjective well-being but also impacts several essential aspects of biological and psychological functioning. While acute stress induces several biological effects that act on different time scales, a two-wave stress response is discussed most prominently. First, during an initial first-wave response of the body to a stressful experience, increased activation of the sympathetic nervous system (SNS) amplifies the release of catecholamines (i.e., noradrenaline, dopamine). During a later second-wave response, stress increases a slower and prolonged activity of the hypothalamus pituitary adrenal (HPA) axis, which increases the release of glucocorticoids (i.e., cortisol) into the bloodstream.

The biological link between acute stress and PM functioning may be found in the shared involvement of the hippocampus and the prefrontal cortex (PFC). PM functioning has been shown to depend in parts on the hippocampus (e.g., Adda, Castro, Além-Mar e Silva, de Manreza, & Kashiara, 2008; McDaniel & Einstein, 2011; but see Cona, Bisiacchi, Sartori, & Scarpazza, 2016, for a meta-analysis of neural correlates of PM), which is highly sensitive to effects of acute stress (e.g., Schwabe, Joëls, et al., 2012). Furthermore, both, the fast SNS and the slow HPA-axis stress response are linked closely to the PFC. The fast release of catecholamines reduces the firing of PFC neurons (Arnsten, 2009), whereas the slow HPA activity changes PFC activity via glucocorticoid receptors and alters neural connectivity (Arnsten, 2015). Consequently, stress can affect cognitive processing that has been linked to the PFC, such as working memory (Oei et al., 2006; Schoofs et al., 2009), attention (Chajut & Algom, 2003; Sänger, Bechtold, Schoofs, Blaszkewicz, & Wascher, 2014), and higher-order cognitive control functions like inhibition or cognitive flexibility (e.g., Plessow, Fischer, et al., 2011; for meta-analyses, see Shields et al., 2015, 2016) that are also involved in PM (e.g., Kliegel et al., 2002; Schnitzspahn, Stahl, Zeintl, Kaller, & Kliegel, 2013).

At the same time, recent neuroimaging studies found PM performance and monitoring for PM cues to be associated with neural activity predominantly in anterior PFC (e.g., Cona et al., 2015; McDaniel, LaMontagne, Beck, Scullin, & Braver, 2013; McDaniel et al., 2015; Underwood, Guynn, & Cohen, 2015), and also linked intention deactivation to PFC functioning (Beck et al., 2014). Furthermore, patients with lesions especially in the rostral PFC have been documented to suffer from severe PM deficits, such as detecting PM cues or resuming task performance after interruptions (Uretzky & Gilboa, 2010).

While the biological link between PM and PFC functioning provides a potential pathway through which acute stress might affect PM and intention deactivation, the direction of the potential effects of acute stress on PM (as well as other higher-order cognitive functions) is not clear. On the one hand, stress-related changes in PFC functioning have been argued to impair higher-order cognitive functions (Arnsten, 2015; Liston, McEwen, & Casey, 2009). On the other hand, increases in cortisol levels have occasionally been reported to benefit certain cognitive-control functions like response inhibition (Schwabe, Höffken, Tegenthoff, & Wolf, 2013; Shields et al., 2016) or goal shielding (Plessow, Fischer, Kirschbaum, & Goschke, 2011). Given the frequently reported effects of stress on cognitive functions and the strong reliance of PM on cognitive control, it is surprising that to our knowledge PM performance in simple, event-based PM tasks (e.g., correct identification and response to a pre-specified PM cue) seems rather robust and mostly unaffected by acute stress (Nater et al., 2006; Walser et al., 2013) as well as chronic stress (McLennan, Ihle, Steudte-Schmiedgen, Kirschbaum, & Kliegel, 2016). For instance, Walser et al. (2013) found preserved PM performance under acute stress, when participants were required to interrupt an ongoing word-categorization task in response to certain pre-specified PM words. Moreover, in their study, acute stress did not affect performance costs when searching for a PM cue (i.e., PM-monitoring costs; Einstein & McDaniel, 2010; R. E. Smith et al., 2007), or interference from no-longer-relevant PM cues (i.e., $\text{PM}_{\text{REPEATED}}$ cues) compared to control stimuli after intention completion (i.e., aftereffects of completed intentions). Yet, concluding invulnerability of PM to acute stress might be premature. It is important to note that previous studies on the effects of acute stress on PM featured only a limited section of PM components and low PM demands that might have concealed or overlooked stress effects on other processes that are involved in PM performance and intention deactivation. Both, Nater et al. (2006) and Walser et al. (2013), for example, employed so-called focal PM cues in which identifying the PM cue (e.g., the word *CANDLE*) required similar processing to performing the ongoing task (e.g., an animate–inanimate decision on the word CANDLE, see Walser et al., 2013). The choice of focal PM cues most likely fostered spontaneous intention retrieval (Einstein et al., 2005; Harrison & Einstein, 2010) and placed rather little demands on PM monitoring (Cona et al., 2016; Scullin, McDaniel, Shelton, & Lee, 2010) or cognitive control (Cona et al., 2015). Since acute stress has been shown to affect cognitive functions under high demands in particular (Oei et al., 2006; Plessow, Schade, Kirschbaum, & Fischer, 2012; Schoofs et al., 2009), it is feasible that PM-task demands in previous studies were simply too low to make potential stress effects visible.

Therefore, in the present study we tested whether an acute stress experience affects PM performance and intention deactivation after intention completion when demands on PM, monitoring, and intention deactivation are increased. The present study was modeled after the approach of Walser et al. (2013) but included two separate conditions:

First, to induce high demands on PM performance and PM monitoring, we specifically used nonfocal PM cues in an event-based PM task in which identifying the PM cue (e.g., the letter sequence *TRA in the word WELTRAUM*) required different stimulus processing than performing the ongoing task (i.e., categorize words as nouns or verbs) (e.g., Einstein et al., 2005). Nonfocal, compared to focal PM cues have repeatedly been shown to increase difficulty of PM-cue detection (i.e., reduced PM accuracy, e.g., Einstein et al., 2005) and strongly engage cognitive-control processes (Cona et al., 2015). Within this *nonfocal condition* we included a two-step parametric manipulation of prospective load (low vs. high) to additionally target different degrees of PM monitoring (A.-L. Cohen, Jaudas, & Gollwitzer, 2008; Meier & Zimmermann, 2015) under already high demands. In the low-load nonfocal condition, participants were instructed to monitor for PM-cue words that contained *one* specific three-letter sequence (e.g., *TRA*: *WELTRAUM*). In the high-load nonfocal condition, however, participants were instructed to monitor for PM-cue words that contained *any permutation* of three specific letters (e.g., *A, C, and K*: *JACKE*).

In the second condition, we induced high demands on intention deactivation by using focal but salient PM cues (and $\mathrm{PM}_{\mathrm{REPEATED}}$ cues), i.e., words that were presented in a distinctive color. we termed this the *focal condition*. This reasoning was based on previous studies on aftereffects of completed intentions showing that the heightened salience of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues can increase the difficulty of intention deactivation (Scullin et al., 2012, 2011). Additionally, we increased PM-monitoring demands compared to earlier studies, by having participants maintain four focal PM cue words instead one (Nater et al., 2006) or two (Walser et al., 2013)[6].

Three hypotheses can be formulated regarding how acute stress may affect PM performance: 1) Acute stress impairs PM functioning (Arnsten, 2015). 2) Acute stress causes shifts in processing strategies to compensate for stress-induced processing challenges (Robert & Hockey, 1997; Steinhauser, Maier, & Hübner, 2007). 3) Acute stress does not impact on PM functioning (Nater et al., 2006; Walser et al., 2013).

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[6] Note that increasing the number of focal PM cues to be maintained still requires considerably less monitoring than the implementation of nonfocal PM cues.

With respect to Hypothesis 1, PM processes that require cognitive control and thus rely on intact PFC functioning should become less efficient. From this perspective, an acute stress experience should reveal negative effects on PM monitoring and performance. PM-cue detection should be impaired, and monitoring costs should be high due to the difficulty to recruit sufficient resources for control-dependent monitoring processes. In addition, less efficient cognitive control mechanisms should impair cognitive control over intention retrieval after intention completion (Bugg et al., 2016). As a consequence, impaired intention deactivation after intention completion should exacerbate interference of $\text{PM}_{\text{REPEATED}}$ cues with ongoing-task processing and/or increase the probability that $\text{PM}_{\text{REPEATED}}$ cues interfere with ongoing-task processing.

Regarding Hypothesis 2, acute stress does not necessarily impair cognitive control functions but may be responsible for shifts in processing strategy, that reflect compensatory effects of resource re-allocation, to optimize performance under the stress-imposed resource constraints (Robert & Hockey, 1997; Steinhauser et al., 2007). In dual-task studies, acute stress resulted in the adoption of resource-saving processing strategies (Plessow et al., 2012) and more efficient processing of both tasks as indicated by increased response speed (Beste, Yildiz, Meissner, & Wolf, 2013). Therefore, under the assumption of a compensatory shift in task processing strategy it is conceivable to assume an apparent optimization strategy. This could be evident in either priority shifts between ongoing task and PM performance and/or in an attempt to adopt a resource-saving task-processing strategy (e.g., to reduce resource-demanding PM monitoring).

Importantly, for both hypotheses—impairment and strategy shifts—the impact of an acute stressor on PM monitoring and PM performance should be more pronounced with higher demands of PM-related processing, e.g., rather under nonfocal than under focal conditions and under high prospective load than under low prospective load. Hypothesis 3 holds that despite the substantial increases of PM-performance demands in the present paradigm the targeted processes involved in PM performance are not susceptible to an acute stressor.

To test these hypotheses, we adopted a research design that targets three important components of PM functioning: PM performance, monitoring, and intention deactivation. For this, participants performed several cycles of a PM block and a test block. PM blocks served to assess PM performance. Participants categorized words during an ongoing word-categorization task and were instructed to press a specific button in response to PM cues instead of performing word categorizations (PM task; see Figure 7). Test blocks did not contain a PM task and served as a no-intention (i.e., ongoing-task-only) baseline for investigating PM-monitoring costs and intention deactivation after PM-task completion. Following random group assignment, half of the participants were exposed to the Trier Social Stress Test (TSST) (Kirschbaum, Pirke, & Hellhammer, 1993), while the remaining half were exposed to a standardized no-stress control procedure (Het, Rohleder, Schoofs, Kirschbaum, & Wolf, 2009). During the TSST, participants first sat through an anticipatory period (5 min) and subsequently engaged in a simulated job interview (5 min), followed by performing mental arithmetic (5 min) in front of a two-person committee and a video camera. During the no-stress control treatment, participants sat through an anticipatory period (5 min) as well, but in contrast to the TSST subsequently engaged in loud speaking (5 min) and mental arithmetic (5 min) without the presence of a committee or a video camera.

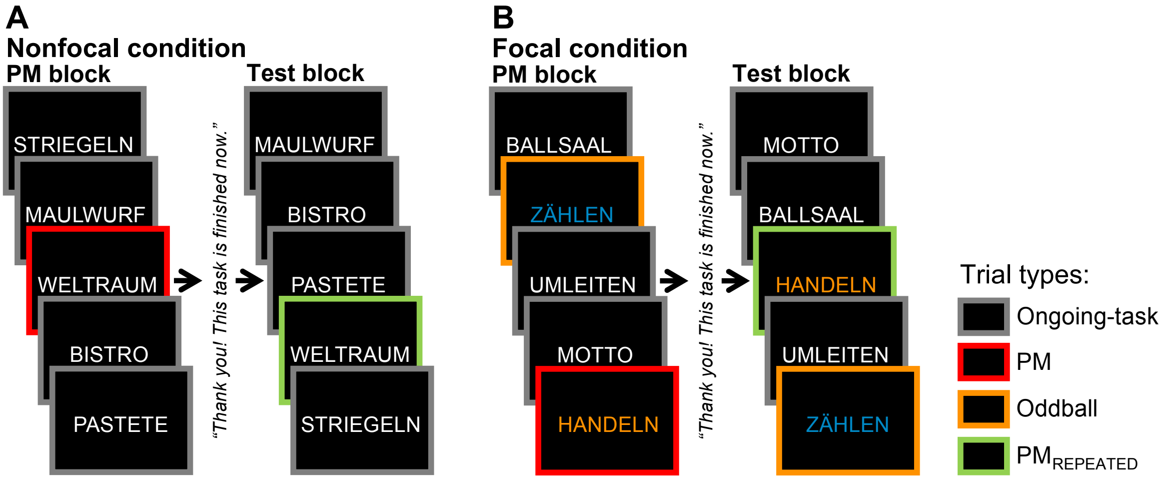

Figure 7. Cognitive task. Illustration of trial sequences during PM and test blocks in the (A) nonfocal and (B) focal condition. Participants categorized German words as nouns versus verbs (ongoing task). In PM trials, they were to press the spacebar instead of performing the ongoing task. In the nonfocal condition, PM cues were words that contained one specific three-letter sequence (e.g., TRA; low prospective load) or any permutation of three specific letters (e.g., T, R, & A; high prospective load). In the focal condition, PM cues were words that were presented in a distinctive color, e.g., HANDELN, printed in orange. Monitoring costs were assessed by comparing performance in ongoing-task trials during PM and test blocks. Aftereffects of completed intentions were assessed by comparing performance in $\text{PM}_{\text{REPEATED}}$ trials to performance in ongoing-task (nonfocal condition) or oddball trials (focal condition) during test blocks. The colored framing was absent during the experiment and is for illustration purposes only.

4.3. Methods

4.3.1. Participants

Eighty participants (40 female, M_{age} = 22.69, SD_{age} = 3.24 years) were tested during a single session, lasting approximately 2 h in exchange for €16. For pragmatic considerations, sample size was based on a previous study using a similar paradigm (Walser et al., 2013). Sample size allowed us to detect medium–large effects of group (f = 0.32, e.g., Faul, Erdfelder, Lang, & Buchner, 2007) at $\alpha = 0.05$ (two-sided) with a power of 0.8.

To reduce individual variability of cortisol levels and stress responses, we only included healthy participants, who were nonsmokers (Rohleder & Kirschbaum, 2006), did not take hormonal contraception (Kirschbaum, Kudielka, Gaab, Schommer, & Hellhammer, 1999), were medication free and of normal weight (BMI: M = 22, SD = 2.55 kg/m^2) and scheduled all visits with a session start between 11.45 am and 5.30 pm (Kudielka, Hellhammer, & Wüst, 2009). Since distribution of testing time did not differ significantly between groups (stress: *Mdn* = 3.38 pm, no-stress: *Mdn* = 2.22 pm), Mann–Whitney U = 661.00, p = .180, it seems unlikely that potential stress effects could be masked by group-differences in circadian variability of PM performance (e.g., Rothen & Meier, 2017).

4.3.2. Ethics Statement

The study was approved by the Institutional Review Board of the Technische Universität Dresden and conducted in accordance to ethical standards of the 1964 Declaration of Helsinki. Informed consent was obtained from all participants prior to performing any study procedures.

4.3.3. Stress Induction and Stress Validation

Half of the participants (20 female, 20 male) were randomly assigned to a stress treatment during which they were exposed to the TSST, a standardized stress-induction protocol administering an experience of uncontrollability and social-evaluative threat (Kirschbaum et al., 1993). The remaining half of the participants was assigned to a no-stress treatment during which they performed a control situation without stress-inducing features (Het et al., 2009).

In order to validate successful stress induction, we analyzed levels of salivary α-amylase (sAA) as a marker of SNS activity (e.g., Granger, Kivlighan, El-Sheikh, Gordis, & Stroud, 2007; Thoma, Kirschbaum, Wolf, & Rohleder, 2012; but see Bosch, Veerman, de

Geus, & Proctor, 2011; Nater & Rohleder, 2009, for discussions about sAA's reflection of SNS activity) and assessed salivary free cortisol as a marker of HPA-axis activity (e.g., Hellhammer, Wüst, & Kudielka, 2009; Kirschbaum & Hellhammer, 1994). Saliva samples were collected 10 min and 1 min prior to treatment, as well as 1, 10, 20, 30, 40, and 50 min after treatment using Salivette® sampling devices (Sarstedt, Nümbrecht, Germany; see Figure 8 A). SAA and cortisol levels were analyzed with quantitative enzyme-kinetic methods (reagents nr. 11876473316 & 10759350-190, Roche Diagnostics Deutschland GmbH; sensitivity: 2 U/ml) (Rohleder & Nater, 2009) and chemiluminescence immunoassays (CLIA, IBL International, Hamburg, Germany; sensitivity: 0.2 nmol/l) with intra- and interassay variabilities ⩽ 6%, respectively.

To ensure that potential treatment effects are truly related to acute stress and not to variations in the participants' mood, arousal or fatigue, we also collected subjective ratings of the participants' good mood, calmness, and alertness over the course of testing via the German *Mehrdimensionaler Befindlichkeitsfragebogen* (MDBF; English version: Multidimensional mental-state questionnaire; Steyer, Schwenkmezger, Notz, & Eid, 1997). Subjective mood data was collected during saliva sampling 10 min prior to treatment, approximately 3 min into treatment (MDBF after saliva sampling) as well as 1, 10, 30, and 50 min after treatment.

4.3.4. Cognitive Task

Apparatus and stimuli. The experiment was performed on a personal computer running Presentation® software (Version 16.3, **www.neurobs.com**). Stimuli (Arial font, size: 24 pt, vertical visual angle ≈ 0.72°) were presented centrally, in white against a black background on a 19-inch monitor at a resolution of 1280 × 1024 pixels (viewing distance ca. 60 cm). A QWERTZ keyboard was used to record the participants' responses. Participants were instructed to press the *X* key with their left index finger to indicate verbs, and the *period* key with their right index finger to indicate nouns.

Target stimuli were 744 German nouns and verbs (50% each; mean word length: 8 letters, range: 5–12 letters; word frequency[7]: $M = 15$, $SD = 3$, range: 6–22; see Appendix B) that were drawn from a pool of German infinitives in written language and did not contain immediate letter repetitions (DeReWo, 2012).

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[7] Word frequency is measured relative to the most frequent word. That is, words with a frequency N are 2N times less frequent than the most frequent word (i.e., German articles "der, die, das" [the] in the DeReWo [2012] word list).

In the *nonfocal condition*, words with one of six pre-specified target syllables (e.g., *TRA* in *WELTRAUM*) served as PM cues. Ongoing-task stimuli consisted of 180 standard words that did not contain any two- or three-letter combinations of either target syllables that were used throughout the experiment (e.g., *PASTETE*). In addition, in order to increase monitoring demands, we included 180 so-called distractor words that contained the first two letters of a given target syllable (e.g., *TR* in *BISTRO*; see Appendix B, Table B1).

In the *focal condition*, PM-cues and oddballs were 40 words (20 each) that did not contain letter combinations of either nonfocal target syllables (see Appendix B, Table B2). Each oddball word was matched to a corresponding PM-cue word in regard to word type, frequency, and length. Focal PM cues and their corresponding oddballs were made salient by presenting them in complementary colors (e.g., light blue–light orange; see Appendix B, Table B5). We included salient oddball cues to disentangle aftereffects of completed intentions in $\text{PM}_{\text{REPEATED}}$ trials from orientation reactions to deviant stimuli (see also, Walser et al., 2012). Ongoing-task stimuli were 280 non-colored standard words.

Task procedure. At the beginning of the experiment, participants practiced the word-categorization task for 40 trials (32 standard, 8 oddball; see Appendix B, Table B3 for the words used). During this initial practice, 8 words were selected randomly to serve as oddball trials and were presented in two complementary colors. Prior to treatment (i.e., stress/no-stress), participants trained the cognitive task for three cycles, each consisting of a PM block and a test block. After treatment and a subsequent 10 min waiting period, participants performed eight cycles of PM block and test block (see Figure 8 B). we included this post-treatment waiting period in order to account for the delayed release of cortisol in response to an acute stress experience with peak cortisol levels between 10–30 min after treatment (e.g., Engert et al., 2011; Kirschbaum & Hellhammer, 2007) and to capture effects of increased HPA-activity on PM functioning after SNS activity and subjective mood should have returned to the level of non-stressed individuals.

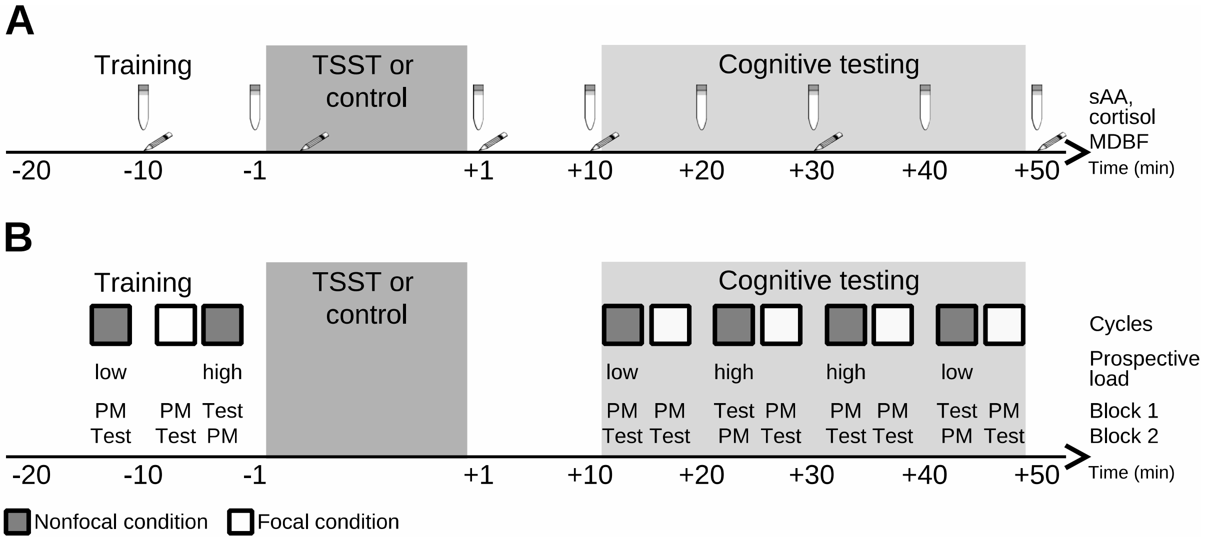

Figure 8. Procedure. (A) Schematic outline of the procedure, starting with a practice of the cognitive task, followed by treatment (i.e., either Trier Social Stress Test [TSST], or a standardized no-stress control treatment) and cognitive testing. Time points for the measurement of salivary α-amylase (sAA), cortisol, and the German MDBF questionnaire (Mehrdimensionaler Befindlichkeitsfragebogen; English version: Steyer et al., 1997) are illustrated. Note that the saliva sample at time point -1 min was taken prior to treatment, while the MDBF was completed after treatment instructions in order to assess participants' anticipation of the upcoming treatment. (B) Example outline of the procedure in the cognitive task. PM block and test block formed a cycle. Cycles are illustrated in grey (nonfocal condition) and white (focal condition) squares. For the training phase, participants in each group were assigned one of two cycle sequences: 1) a low-load nonfocal cycle, followed by a focal and a high-load nonfocal cycle, or 2) a high-load nonfocal cycle, followed by a focal and a low-load nonfocal cycle. During cognitive testing, participants alternated between focal and nonfocal cycles. One half of the participants in either group started with a nonfocal condition and the other half with a focal condition. Low and high prospective loads were assigned randomly to nonfocal cycles. To avoid overestimating the size of monitoring costs in the nonfocal condition, half of the cycles (1 low-load, 1 high-load) started with a PM block, while the remaining half started with a test block. In the focal condition, however, all cycles started with the PM block to maximize statistical power for the assessment of aftereffects of completed intentions.

In PM blocks, participants categorized words as either verb or noun (ongoing task). Participants were instructed to press the spacebar with either thumb instead of performing the word categorization whenever they encountered a PM cue. PM cues differed for the focal, the low-load and high-load nonfocal conditions. In particular, participants were instructed to press the space bar in response to (a) any one of four pre-specified words that additionally differed from ongoing-task words in their color, e.g., *HANDELN* printed in orange (focal PM task); (b) any word that contained a pre-specified sequence of three letters, such as *TRA* in the word *WELTRAUM* (low-load nonfocal PM task); or (c) any word that contained a random permutation of three pre-specified, subsequent letters, such as *A*, *C*, and *K* in *JACKE* (high-load nonfocal PM task).

Low-load and high-load nonfocal conditions differed only in regard to the PM-task instructions but not in PM cues. That is, although high-load nonfocal PM tasks instructed to monitor for permutations of a letter string (e.g., permutations of *A*, *C*, & *K*), only one permutation was presented within a cycle (e.g., *ACK* in the word *JACKE*). In order to support understanding of the task instructions, prior to testing, instructions for each PM-task type were recalled verbally by the participants once and were subsequently reiterated by the experimenter, who also provided examples for PM tasks in the nonfocal condition.

Task instructions were displayed for either 20 s (PM block) or 8 s (test block) at the beginning of each block. At the end of the first block of each cycle, participants were informed that the current task was finished (i.e., "*Thank you! This task is now finished*".; 4 s). After treatment, in addition to breaks due to collection of saliva samples, further breaks of 6 s were provided between cycles.

Each PM and test block contained 64 trials of the word-categorization task (50% nouns; focal condition: 56 ongoing-task, 4 PM, 4 oddball; nonfocal condition: 60 ongoing-task, 4 PM). Trials started with the presentation of a fixation cross (plus sign, font size = 18 points) for 400 ms. Imperative stimuli were displayed until a response was given or for a maximum of 3000 ms, followed by a blank screen for 140 ms. Errors and response omissions prompted a feedback tone (785 Hz, 140 ms) through headphones during the blank screen. Trials were presented in a random fashion, with PM or $\mathrm{PM}_{\mathrm{REPEATED}}$ cues being randomly interspersed after the first four trials of each PM block or test block, respectively (focal condition: 4 standard words; nonfocal condition: 2 standard, 2 distractor words). In order to control for variability in both speed and difficulty of reading and categorizing target words, the same words were presented during PM and test block of a given cycle (see Walser et al., 2013 for a similar design).

Standard words were selected randomly without replacement for each cycle from a pool of 460 words (see Appendix B, Table B4). Focal PM cues and oddballs were selected randomly without replacement from a pool of 20 PM-cue–oddball word pairs and were assigned randomly to focal cycles for each participant. Colors for PM cues and oddballs in the focal condition were randomly drawn from a pool of 6 complementary-color pairs and randomly assigned to the initial task practice and subsequent focal cycles for each participant. PM-cue and oddball colors remained constant throughout a cycle but changed between cycles in the focal condition. For nonfocal PM tasks, PM cues together with their corresponding set of distractor words were assigned randomly to nonfocal cycles for each participant.

4.3.5. Procedure

At the beginning of the session, participants supplied basic demographic information and trained the cognitive task. After treatment (approximately 40 min after session start), participants performed the experimental cognitive task (from 10 min to 50 min after treatment completion). In order to reduce variability in participants' blood-glucose levels that can affect the size of stress-induced increases in HPA activity, participants were instructed to refrain from drinking sweetened beverages and eating for at least 1.5 hours prior to testing and were given 200 ml grape juice in an attempt to standardize blood-glucose levels at the beginning of the session (Kirschbaum et al., 1997).

4.3.6. Data Trimming

For RT analyses of PM performance and aftereffects of completed intentions, we excluded errors (4.4%) and trials with RTs above or below 2.5 SDs of a participants mean in a given focality condition (focal, nonfocal), prospective load (low, high), block (PM block, test block), and trial type[8] (ongoing-task [separate for standard and distractor words], oddball, PM, $\text{PM}_{\text{REPEATED}}$; 3.0%). For the analysis of PM monitoring costs, we first excluded two post-PM trials (5.9%) (Brewer, 2011) to reduce variance from aftereffects of responding to PM cues on task performance (Meier & Rey-Mermet, 2012) and subsequently applied the same data trimming procedure as above for RT analyses, resulting in the exclusion of 4.4% errors and 2.9% outlier trials. Error analyses were conducted separately for erroneous PM responses during PM blocks (i.e., false alarms) and during test blocks (i.e., commission errors) as well as for the remaining errors, including response omissions.

4.4. Results

4.4.1. Stress Response

In order to validate the physiological and subjective consequences of treatment, we examined the time course of sAA and cortisol levels as well as the participants' mental state in repeated measures ANOVAs involving the factors group (stress vs. no-stress) and time on sAA and cortisol data (see Figure 9). To determine at which time points treatment effects differed between groups, we additionally conducted Bonferroni-corrected post-hoc *t* tests for group differences at each time point. Note, that *p*-values for which the Bonferroni-corrected alpha level applies are denoted as p^*.

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[8] Note that results did not change when refraining from outlier correction.

sAA. Stress and no-stress group differed in sAA time course, $F(7, 518) = 8.36$, $p < .001$, $\eta_p^2 = .10$. Post-hoc *t* tests at a Bonferroni-corrected alpha level of .006 (.05/8) revealed no effect of treatment on sAA levels at −10 min, $t(78) = .563$, $p^* = .575$, $d_s = 0.13$, or at −1 min prior to treatment, $t(79) = 1.94$, $p^* = .055$, d_s[9] $= 0.44$. Most importantly, the stress group showed significantly higher sAA levels only immediately after treatment (1 min), $t(79) = 3.29$, $p^* = .002$, $d_s = 0.74$. Amylase levels were nominally increased at time points +10 min, $t(79) = 1.90$, $p^* = .061$, $d_s = 0.43$, and +40 min after treatment, $t(79) = 2.00$, $p^* = .049$, $d_s = 0.45$. Throughout the remainder of the session, sAA levels did not differ between groups, p^*s $\geqslant .144$, $-0.01 \leqslant d_s$s $\leqslant 0.33$.

Cortisol. Time course of cortisol levels differed between groups, as indicated by a significant Time × Group interaction, $F(7, 546) = 24.37$, $p < .001$, $\eta_p^2 = .24$. Post-hoc *t* tests (Bonferroni-corrected $\alpha = .006$ [.05/8]) revealed that prior to treatment cortisol levels did not differ between groups, p^*s $\geqslant .467$, $-0.16 \leqslant d_s$s $\leqslant -0.06$. We also found a descriptive but not statistically significant treatment effect on cortisol levels at time point +1 min after treatment, $t(78) = 2.24$, $p^* = .028$, $d_s = 0.50$. For all other time points following treatment, cortisol levels were higher in the stress group than the no-stress group, p^*s $\leqslant .001$, $1.00 \leqslant d_s$s $\leqslant 1.17$.

Mental state. Post-hoc *t* tests were conducted at a Bonferroni-corrected alpha level of .008 (.05/6). Group affected the time courses of good mood, $F(5, 385) = 11.17$, $p < .001$, $\eta_p^2 = .13$. The stress group reported worse mood after having received instructions for the upcoming treatment (−1 min), $t(78) = -2.77$, $p^* = .007$, $d_s = -0.62$, as well as immediately after, $t(78) = -4.25$, $p^* < .001$, $d_s = -0.95$, and +10 min after treatment, $t(78) = -3.03$, $p = .003$, $d_s = -0.68$. The other time points (including baseline at −10 min prior to treatment) did not differ between groups, p^*s $\geqslant .184$, $-0.30 \leqslant d_s$s $\leqslant -0.12$. Time courses of calmness were also affected by group, $F(5, 385) = 18.59$, $p < .001$, $\eta_p^2 = .19$. The stress group reported decreased calmness after having received instructions for the upcoming treatment (3 min into treatment), $t(78) = -3.33$, $p^* = .001$, $d_s = -0.74$, and immediately after treatment, $t(78) = -4.69$, $p^* < .001$, $d_s = -1.05$. At time point +10 min after treatment, calmness was only nominally reduced in the stress group, $t(78) = -2.56$, $p^* = .012$, $d_s = -0.57$. At all other time points, calmness did not differ between groups, p^*s $\geqslant .354$, $-0.02 \leqslant d_s$s $\leqslant 0.21$. Lastly, we

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[9] Effect sizes for mean differences were computed according to Lakens (2013) and refer to the standardized between-subject (d_s) or within-subject (d_{rm}) mean differences.

found similar time courses of alertness between groups, $F(5, 385) = 1.18$, $p = .319$, $\eta_p^2 = .01$, $-0.29 \leqslant d_s\text{s} \leqslant 0.03$.

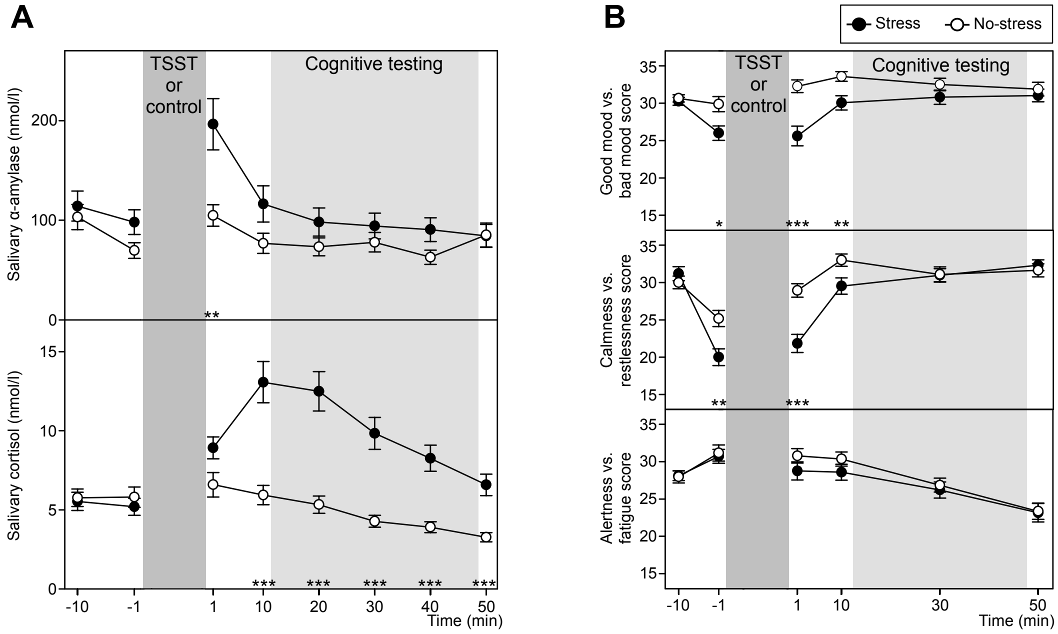

Figure 9. Stress response. Time courses of (A) mean salivary α-amylase (sAA) and cortisol levels as well as of (B) mean mental-state scores on the three subscales good mood vs. bad mood, calmness vs. restlessness, and alertness vs. fatigue of the German Mehrdimensionaler Befindlichkeitsfragebogen (MDBF; English version: Multidimensional mental-state questionnaire; Steyer et al., 1997) during the experimental session (minutes before or after Trier Social Stress Test [TSST] or control treatment, respectively) for the stress and the no-stress group. Error bars represent standard errors of the mean. $^*p < .05$, $^{**}p < .01$, $^{***}p < .001$, for Bonferroni-adjusted *p* values.

4.4.2. Prospective Memory Functioning

In order to determine statistical significance of the hypotheses-relevant effects—stress effects and a potential demand dependence of stress effects on PM performance, PM-monitoring costs, aftereffects of completed intentions and commission-error risk—we used a Bonferroni-corrected alpha level of .002 per test (.05/24). Specifically, we corrected for overall 24 tests of group differences in the hypotheses-relevant dependent variables. Note, that *p* values for tests at the Bonferroni-corrected alpha level are denoted as p^*.

Nonfocal condition. For PM performance, we conducted mixed ANOVAs involving the factors group (stress vs. no-stress) and prospective load (low vs. high) on RTs and error rates on PM trials and false-alarm PM responses during ongoing-task performance. The effects of interest were a main effect of group and a Group × Prospective load interaction. To examine monitoring costs, we conducted 2 Block (PM vs. test block) × 2 Prospective load (low vs. high) × 2 Group (stress vs. no-stress) ANOVAs on ongoing-task RTs and error rates. Here our main focus lied on the potential interactions of Group × Block and Group × Block ×

Prospective load. For the analysis of aftereffects of completed intentions, we compared performance in $\mathrm{PM}_{\mathrm{REPEATED}}$ and ongoing-task trials during test blocks in 2 Prospective load (low vs. high) × 2 Trial type ($\mathrm{PM}_{\mathrm{REPEATED}}$ vs. ongoing task) × 2 Group (stress vs. no-stress) ANOVAs (see Figure 10). Note that aftereffect analyses in the nonfocal condition were conducted exclusively for test blocks following nonfocal PM blocks, since only these contained $\mathrm{PM}_{\mathrm{REPEATED}}$ trials. In the aftereffect analyses, the effects of interest were a Group × Trial type and a Group × Trial type × load interaction.

PM performance. RTs on nonfocal PM cues did not differ between stress (1197 ms) and no-stress group (1240 ms), $F(1, 78) = .845$, $p^* = .361$, $\eta_p^2 = .01$. PM responses were slower under high (1256 ms) compared to low prospective load (1181 ms), $F(1, 78) = 8.64$, $p = .004$, $\eta_p^2 = .10$. Although this PM-response slowing under high compared to low prospective load was less pronounced in the stress group (43 ms, $d_{\mathrm{rm}} = 0.19$) compared to the no-stress group (108 ms, $d_{\mathrm{rm}} = 0.42$), this difference failed statistical significance, $F(1, 78) = 1.62$, $p^* = .207$, $\eta_p^2 = .02$. Treatment had no statistically significant effect on PM errors (stress: 25.8%; no-stress: 20.8%), $F(1, 78) =1.75$, $p^* = .190$, $\eta_p^2 = .02$, or false-alarm rates (stress: 0.5%; no-stress: 0.4%), $F(1, 78) = .58$, $p^* = .449$, $\eta_p^2 = .01$. No further effects were significant, all Fs $\leqslant$ 2.71, ps $\geqslant .104$, η_p^2s $\leqslant .03$.

Monitoring costs. Monitoring costs were reflected in substantially slower ongoing-task performance during PM blocks (1023 ms) compared to test blocks (704 ms), $F(1, 78) = 419.96$, $p < .001$, $\eta_p^2 = .84$. Most importantly, these monitoring costs were markedly lower in the stress group (269 ms, $d_{rm} = 1.42$) than in the no-stress group (371 ms, $d_{rm} = 1.80$), $F(1, 78) = 10.66$, $p^* = .002$, $\eta_p^2 = .12$ (see Table 6). Further, monitoring costs were more pronounced under high (342 ms, $d_{rm} = 1.59$) compared to low prospective load (297 ms, $d_{rm} = 1.43$), $F(1, 78) = 13.26$, $p < .001$, $\eta_p^2 = .15$. This effect, however, was only numerically smaller in the stress (27 ms, $d_{rm} = 0.19$) than in the no-stress group (64 ms, $d_{rm} = 0.37$), $F(1, 78) = 2.30$, $p^* = .134$, $\eta_p^2 = .03$. Finally, ongoing-task responses were faster in the stress (818 ms) compared to the no-stress group (909 ms), $F(1, 78) = 7.31$, $p = .008$, $\eta_p^2 = .09$, and participants responded slower under high (874 ms) than under low prospective load (853 ms), $F(1, 78) = 10.17$, $p = .002$, $\eta_p^2 = .12$. This load-dependent slowing did not differ between stress (16 ms, $d_{rm} = 0.11$) and no-stress group (27 ms, $d_{rm} = 0.17$), $F(1, 78) = .64$, $p = .427$, $\eta_p^2 = .01$.

Error rates mirrored the RT data and were increased during PM blocks (4.7%) compared to test blocks (3.8%), indicating the presence of monitoring costs during PM task performance, $F(1, 78) = 17.26$, $p < .001$, $\eta_p^2 = .18$. While these costs were again nominally smaller in the stress (0.3%, d_{rm} = 0.09) than in the no-stress group (1.6%, d_{rm} = 0.54), this difference did not reach significance at the Bonferroni-corrected alpha level, F(1, 78) = 8.61, p^* = .004, η_p^2 = .10. Monitoring costs were increased during high (1.6%, d_{rm} = 0.48) compared to low prospective load (0.3%, d_{rm} = 0.09), F(1, 78) = 10.68, p = .002, η_p^2 = .12. This increase in monitoring costs, however, was similar in the stress (1.2%, d_{rm} = 0.42) and in the no-stress group (1.5%, d_{rm} = 0.57), F(1, 78) = .12, p^* = .726, η_p^2 = .00. No further hypotheses-relevant effects were significant, all Fs ⩽ 1.20, ps ⩾ .277, η_p^2s ⩽ .02.

Aftereffects of completed intention. Participants' responses were slower in PMREPEATED (757 ms) compared to ongoing-task trials (695 ms), F(1, 78) = 29.10, p < .001, η_p^2 = .27, which indicated the presence of aftereffects of completed intentions (see Table 7). Most importantly, aftereffects did not differ between groups (stress: 50 ms, d_{rm} = 0.35; no-stress: 73 ms, d_{rm} = 0.38), F(1, 78) = .997, p^* = .321, η_p^2 = .01. We found no further statistically significant effects on RTs, all Fs ⩽ 3.10, ps ⩾ .082, η_p^2s ⩽ .04, or any effects in error rates, all Fs ⩽ 1.97, ps ⩾ .164, η_p^2s ⩽ .03. Since only one participant made a single commission error in the nonfocal condition (stress group, low prospective load) we did not conduct a commission-error analysis.

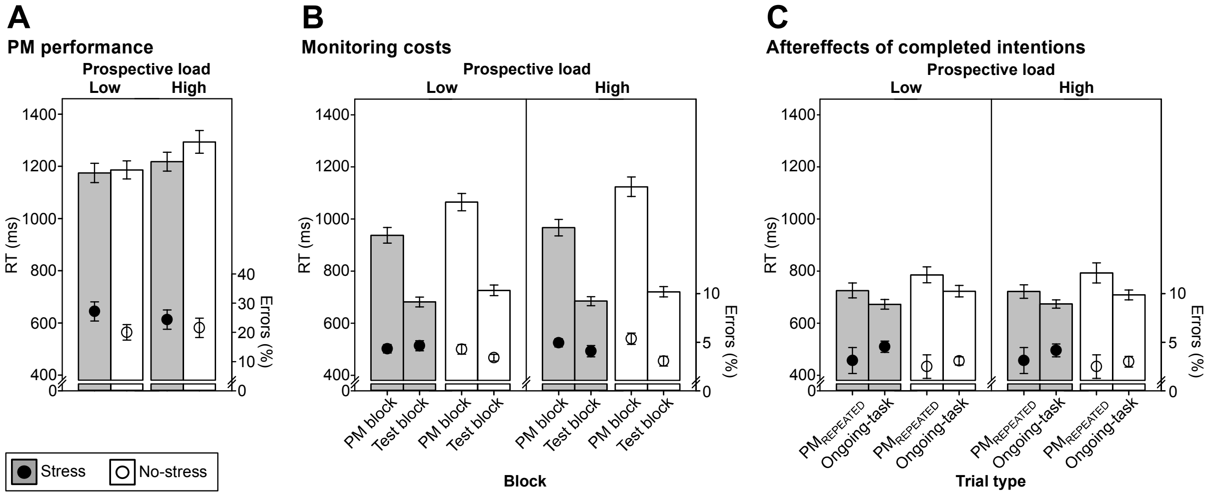

Figure 10. Results of the nonfocal condition. Mean response times (RT) and error rates for (A) PM performance, (B) monitoring costs, and (C) aftereffects of completed intentions as a function of prospective load (low vs. high), group (stress vs. no-stress), block (only for monitoring costs: PM vs. test block), and trial type (only for aftereffects of completed intentions: PMREPEATED vs. oddball). For aftereffects of completed intentions, data were obtained only from test blocks that followed PM blocks, since only these contained PMREPEATED trials. Error bars represent standard errors of the mean.

Focal condition. Analyses of the focal condition were similar to the nonfocal condition but did not include the within-subject factor prospective load. For aftereffect analyses, we contrasted performance on $\mathrm{PM}_{\mathrm{REPEATED}}$ and oddball trials (see Figure 11). As in the analyses for the nonfocal condition, we determined statistical significance for the effects of interest at a Bonferroni-corrected alpha level of .002 per test (.05/24).

PM performance. Analyses did not reveal a significant effect of stress on PM performance, as RTs were similar in the stress (786 ms) and the no-stress group (786 ms), $F(1, 78) < .001$, $p^* = .990$, $\eta_p^2 < .001$, and error rates did not differ between groups (stress: 11.0%; no-stress: 8.3%), $F(1, 78) =1.16$, $p^* = .285$, $\eta_p^2 = .02$. Participants committed more false alarms during oddball (2.7%) than during standard trials (0.1%), $F(1, 78) = 29.23$, $p < .001$, $\eta_p^2 = .27$. All further effects were not significant, Fs ⩽ 1.42, ps ⩾ .238, η_p^2s ⩽ .02.

Monitoring costs. Performance decrements during PM blocks (760 ms) compared to test blocks (668 ms) indicated monitoring costs, $F(1, 78) = 181.75$, $p < .001$, $\eta_p^2 = .70$. However, in contrast to the nonfocal condition, monitoring costs did not differ statistically between stress (87 ms, $d_{rm} = 0.65$) and no-stress group (98 ms, $d_{rm} = 0.66$), $F(1, 78) = .67$, $p^* = .415$, $\eta_p^2 = .01$. Analyses also revealed a small to moderate, but nonsignificant, trend for faster ongoing-task responses in the stress (690 ms) compared to the no-stress group (739 ms), $F(1, 78) = 3.64$, $p = .060$, $\eta_p^2 = .05$ (see Table 6). Error rates did not differ between groups (stress: 3.7%; no-stress: 2.8%), $F(1, 78) = 3.03$, $p = .086$, $\eta_p^2 = .04$, but were lower during PM blocks (3.0%) than during test blocks (3.5%), $F(1, 78) = 7.78$, $p = .007$, $\eta_p^2 = .10$. This block effect, however, did not differ between stress (−0.7%, $d_{rm} = -0.24$) and no-stress group (−0.5%, $d_{rm} = -0.21$), $F(1, 78) = .19$, $p^* = .667$, $\eta_p^2 = .002$.

Aftereffects of completed intentions. Slower responses towards $\mathrm{PM}_{\mathrm{REPEATED}}$ (821 ms) than towards oddball trials (732 ms) indicated the presence of aftereffects of completed intentions, $F(1, 78) = 69.94$, $p < .001$, $\eta_p^2 = .47$ (see Table 7). Aftereffects did not differ between stress (72 ms, $d_{rm} = 0.51$) and no-stress group[10] (106 ms, $d_{rm} = 0.65$), $F(1, 78) = 2.44$, $p^* = .122$, $\eta_p^2 = .03$. Again, the stress group responded numerically faster (754 ms) than the no stress group (800 ms), which did not reach significance, $F(1, 78) = 2.34$, $p = .130$, $\eta_p^2 = .03$. There were no significant effects on error rates, all Fs ⩽ 1.42, ps ⩾ .238, η_p^2s ⩽ .02.

Similar to previous research on commission errors (e.g., Anderson & Einstein, 2017; Scullin et al., 2012), we analyzed the proportion of participants who made at least one

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[10] This result remained unchanged when excluding participants who made commission errors.

commission error on standard, PMREPEATED, or oddball trials in test blocks after treatment[11]. While on a descriptive level, commission errors were more frequently made by participants in the stress (11 participants) than in the no-stress group (3 participants), this difference did not reach the Bonferroni-corrected alpha level of significance, $\chi^2(1) = 5.54$, $p^* = .019$, odds ratio = 4.63.

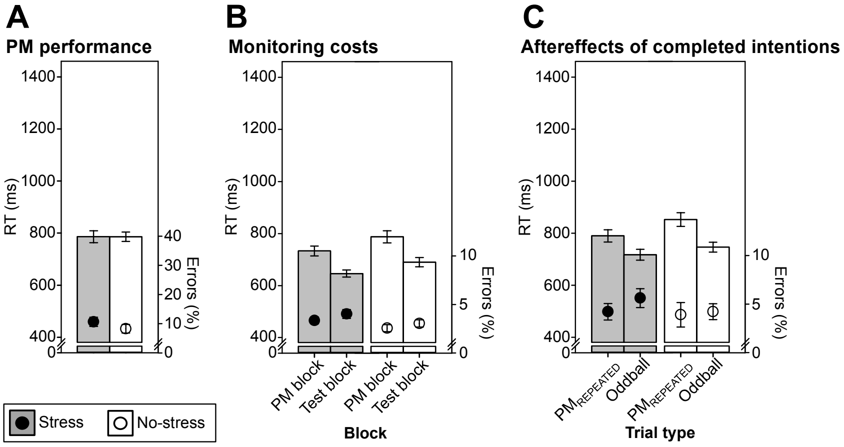

Figure 11. Results of the focal condition. Mean response times (RT) and error rates for (A) PM performance, (B) monitoring costs, and (C) aftereffects of completed intentions as a function of group (stress vs. no-stress), block (only for monitoring costs: PM block vs. test block), and trial type (only for aftereffects of completed intentions: PMREPEATED vs. oddball). Error bars represent standard errors of the mean.

Comparison of focal and nonfocal condition. In order to determine whether stress differentially affected monitoring costs in the focal compared to the nonfocal condition, we conducted mixed ANOVAs involving the factors group (stress vs. no-stress) and PM condition (focal vs. nonfocal) on monitoring costs in RTs and error rates (averaged across low and high prospective load in the nonfocal condition). For RTs, monitoring costs were increased in the nonfocal (320 ms) compared to the focal condition (92 ms), $F(1, 78) = 280.94$, $p < .001$, $\eta_p^2 = .78$. Most importantly, a significant Group × PM condition interaction, $F(1, 78) = 11.15$, $p^* = .001$, $\eta_p^2 = .13$, revealed a more pronounced reduction of monitoring costs under stress in the nonfocal (102 ms) than in the focal condition (11 ms). For error rates, we also found larger monitoring costs in the nonfocal (0.9%) compared to the focal condition (−0.6%), $F(1, 78) = 22.28$, $p < .001$, $\eta_p^2 = .13$. However, the Block × Group × PM condition interaction did not reach significance, $F(1, 78) = 3.15$, $p^* = .080$, $\eta_p^2 = .04$ (nonfocal: 1.3%, focal: 0.2%).

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[11] Commission errors occurred across all focal cycles after treatment and almost exclusively during the first encounter of a PMREPEATED cue within a block, except for one participant who made a commission error on the first deviant trial that was presented in the block (i.e., oddball trial).

Table 6

Assessment of monitoring costs. PM and ongoing-task performance in mean RTs, error rates, and false-alarm PM responses during PM and test blocks by trial type (ongoing task, PM, oddball) (standard deviations in parentheses).

		Stress group			**No-stress group**		
Trial type		**RT (ms)**	**Errors (%)**	**False PM (%)**	**RT (ms)**	**Errors (%)**	**False PM (%)**
Nonfocal condition, low prospective load							
PM block	PM	1175 (233)	27.2 (20.8)	-	1186 (219)	20.0 (16.9)	-
	Ongoing task	937 (191)	4.3 (2.7)	0.4(0.6)	1065 (211)	4.3 (3.2)	0.4 (0.7)
Test block	Ongoing task	682 (117)	4.7 (3.2)	0	726 (127)	3.4 (2.2)	0
Monitoring costs[a]		256 (119)	−0.3 (2.9)	-	338 (141)	0.9 (2.4)	-
Nonfocal condition, high prospective load							
PM block	PM	1218 (228)	24.4 (21.0)	-	1294 (276)	21.6 (20.8)	-
	Ongoing task	967 (200)	4.9 (2.7)	0.7 (0.9)	1124 (238)	5.4 (3.7)	0.5 (0.8)
Test block	Ongoing task	685 (110)	4.1 (3.6)	0	721 (125)	3.1 (3.2)	0
Monitoring costs[a]		282 (141)	0.9 (2.7)	-	403 (191)	2.3 (2.7)	-
Focal condition							
PM block	PM	786 (142)	10.6 (10.0)	-	786 (113)	8.3 (9.4)	-
	Oddball	1054 (204)	8.0 (6.3)	3.3 (5.1)	1104 (191)	7.5 (7.00)	2.2 (3.6)
	Ongoing task	733 (122)	3.4 (2.2)	0.0 (0.1)	787 (146)	2.6 (2.3)	0.1 (0.22)
Test block	Ongoing task	647 (86)	4.0 (2.9)	0	690 (113)	3.0 (2.4)	0
Monitoring costs[a]		86 (55)	−0.7 (1.8)	-	97 (66)	−0.5 (1.9)	-

[a] Monitoring costs represent differences in ongoing-task performance between PM blocks and test blocks.

Table 7

Assessment of aftereffects of completed intentions. Mean RTs, error rates and false-alarm PM responses during test blocks by trial type (ongoing task, PM$_{\text{REPEATED}}$, oddball) (standard deviations in parentheses).

		Stress group			**No-stress group**		
Trial type		**RT (ms)**	**Errors (%)**	**False PM (%)**	**RT (ms)**	**Errors (%)**	**False PM (%)**
Nonfocal condition, low prospective load[a]							
Test block	Ongoing task	673 (182)	4.5 (3.6)	0	723 (141)	3.1 (2.7)	0
	PM$_{\text{REPEATED}}$	726 (182)	3.1 (8.4)	0	793 (244)	2.5 (7.6)	0
Aftereffects[b]		53 (120)	−1.4 (8.6)	-	63 (140)	−0.6 (7.0)	-
Nonfocal condition, high prospective load[a]							
Test block	Ongoing task	674 (100)	4.2 (4.2)	0	709 (118)	3.0 (3.5)	0
	PM$_{\text{REPEATED}}$	723 (161)	3.1 (8.4)	0.3 (4.0)	793 (196)	2.5 (7.6)	0
Aftereffects[b]		48 (116)	−1.0 (8.3)	-	84 (180)	−0.5 (8.4)	-
Focal condition							
Test block	Oddball	717 (130)	5.6 (6.1)	0	747 (120)	4.2 (5.2)	0
	PM$_{\text{REPEATED}}$	790 (150)	4.2 (5.4)	1.6 (2.7)	852 (166)	3.9 (8.0	0.5 (1.7)
Aftereffects[b]		72 (98)	−1.4 (6.6)	-	106 (92)	−0.3 (7.6)	-

[a] Data for the nonfocal condition was obtained from test blocks that followed PM blocks only. Since only these contained PM$_{\text{REPEATED}}$ trials.

[b] Aftereffects represent performance differences between PM$_{\text{REPEATED}}$ and ongoing-task (nonfocal condition) or oddball trials (focal condition).

4.5. Discussion

In the present study, we investigated the effects of acute stress on PM performance and intention deactivation. In two conditions, we parametrically varied the demands on PM monitoring (nonfocal condition) or increased the demands on successful intention deactivation (focal condition). With this, we tested the following hypotheses: 1) Acute stress impairs PM and intention deactivation. 2) Acute stress induces a shift in performance strategies that could either manifest itself in terms of task-priority changes or in terms of a shift towards less resource-consuming strategies to uphold PM performance and intention deactivation. 3) Acute stress does not affect PM and intention deactivation.

First of all, our results yielded evidence for a successful stress induction. An immediate short-lived increase in SNS activity (i.e., increased sAA levels), a slower, long-lasting increase in HPA-axis activity (increased cortisol levels), as well as immediate subjective consequences (i.e., worse mood, increased restlessness) in the stress compared to the no-stress group after treatment indicated valid biological and subjective stress responses. Increased SNS activity and subjective treatment consequences dissipated almost completely within 10 min after stressor cessation and treatment had no effect on participants' alertness or fatigue. Therefore, it is unlikely that the observed stress effects on PM functioning could be explained by differences in these variables (see also, Plessow, Fischer, et al., 2011; Plessow, Kiesel, & Kirschbaum, 2011; Plessow et al., 2012).

Second, the present paradigm revealed the expected PM effects. That is, PM error rates were within the expected range for PM studies (i.e., no ceiling effect occurred), we found evidence for PM-monitoring costs in focal and nonfocal PM tasks, and replicated earlier findings of increased monitoring costs under high, compared to low prospective load in a nonfocal PM task, while prospective load had no effect on PM performance (A.-L. Cohen et al., 2008; Meier & Zimmermann, 2015).

Consistent with PM literature, PM error rates and monitoring costs were smaller in the focal compared to the nonfocal condition (e.g., Einstein et al., 2005). Aftereffects of completed intentions occurred in terms of increased performance costs on $\mathrm{PM}_{\mathrm{REPEATED}}$ compared to ongoing-task (nonfocal condition) or oddball trials (focal condition). Commission errors were rare and predominantly occurred in the focal condition.

Most important are the effects of acute stress on PM functioning and intention deactivation, which can be summarized in three main findings: First, PM performance in either task was not affected by stress. Second, stress had no effect on PM-monitoring costs in the focal condition but led to a substantial reduction of monitoring costs in the nonfocal condition. Third, while aftereffects of completed intentions in terms of performance costs were not affected by stress in both focal and nonfocal conditions, we found more commission errors in the stress group under focal conditions.

Note that this pattern of acute-stress effects on PM and intention deactivation was not predicted by either of our hypotheses. We will now elaborate these findings in more detail.

4.5.1. Effects of Acute Stress on PM Performance

Neither increasing monitoring costs via prospective load, nor decreasing monitoring costs via acute stress affected speed or accuracy of PM performance, which is in line with previous findings (Scullin, McDaniel, & Einstein, 2010; but see R. E. Smith, 2003). Furthermore, similar to previous studies, we did not observe any substantial effect of stress on event-based PM performance when demands on PM-cue detection were low (focal condition) (Nater et al., 2006; Walser et al., 2013). Going beyond previous reports, even under high demands on PM-cue detection (nonfocal condition, high prospective load), acute stress did not lead to changes in PM performance in this study. Furthermore, no ceiling effects were observed in PM performance. Thus, it seems unlikely that potential stress effects on PM were masked by high PM performance.

Note that, although PM performance in the focal condition most likely relied to a great extent on spontaneous intention retrieval rather than on monitoring for PM cues, we nevertheless found evidence for PM monitoring in the focal condition. Therefore, based on the present findings, we cannot rule out that acute stress might affect PM performance that relies solely on spontaneous intention retrieval in the absence of monitoring costs, for instance, when PM cues are extremely rare and salient (e.g., a single appearance of one PM cue) and ongoing-task performance is emphasized (McDaniel et al., 2015). The growing body of evidence that acute stress does not impair memory for stimulus–response links, however, renders this unlikely (Schwabe, Tegenthoff, Höffken, & Wolf, 2012; Schwabe & Wolf, 2009). That is, acute stress has repeatedly been shown to have no negative effect on the encoding and retrieval of stimulus–response links (Schwabe, Tegenthoff, et al., 2012; Schwabe & Wolf, 2009). At the same time, focal PM and, to a lesser extent, also nonfocal PM performance relies on the retrieval of PM-cue–action links that are established during the encoding of PM tasks (Cohen-Kdoshay & Meiran, 2007; Momennejad & Haynes, 2013). Accordingly, taken together with previous reports (Nater et al., 2006; Walser et al., 2013), our findings strongly suggest that intention retrieval is preserved under acute stress. Moreover, since in the present and previous studies (Nater et al., 2006; Walser et al., 2013) both retrieval and encoding of event-based PM tasks took place after stress induction, intention encoding likely is also preserved under acute stress. A recent study by Glienke and Piefke (2016) observed preserved PM performance more than 55 min after acute stress induction, while PM performance of the control group deteriorated. While conceptually and methodologically different to the present study, such findings underscore the importance of future research on the effects of acute stress on PM under different task demands and across longer time frames.

4.5.2. Effects of Acute Stress on PM Monitoring

Effects of acute stress on PM monitoring appeared only in high-demanding monitoring conditions (nonfocal condition). Here, monitoring costs were substantially reduced under acute stress compared to those of non-stressed controls. This finding suggests that under high demands on PM monitoring stress results in an overall more efficient task performance, which might be interpreted as a shift towards performance strategies that reduced the resources or effort needed to uphold successful PM performance (Robert & Hockey, 1997).

These effects of acute stress on PM-monitoring costs were not observed under less demanding focal PM-cue conditions, which is in line with previous studies (Nater et al., 2006; Walser et al., 2013). Although we did observe monitoring costs in the focal condition, we presume that PM performance relied mostly on spontaneous retrieval and placed little overall demand on monitoring (Cona et al., 2016; Einstein et al., 2005). This was also corroborated by the observation of relatively small monitoring costs during focal PM tasks in comparison to large monitoring costs in nonfocal PM tasks.

Interestingly, even though stress effects on monitoring seemed to be contingent upon monitoring demands, they were not affected by our manipulation of prospective load. Two potential explanations are conceivable. First, exploratory analyses of monitoring costs on standard trials (50% of ongoing-task trials) in the nonfocal condition revealed a more pronounced reduction in monitoring costs under stress in the high-load compared to low-load condition. Hence, it might be possible that the use of distractor words in the nonfocal condition (50% of ongoing-task trials contained the first two letters of a given PM-cue syllable) attenuated stress effects on monitoring. Alternatively, manipulations of PM-cue focality and prospective load might affect monitoring costs via different mechanisms. More specifically, nonfocal compared to focal PM cues target different retrieval processes in PM (i.e., top-down controlled vs. bottom-up triggered intention retrieval, McDaniel et al., 2015) and modulate difficulty of PM-cue detection via cue-distinctiveness or perceptual demands on visual search (e.g., McDaniel & Einstein, 2000). Increasing prospective load (i.e., the number of PM cues), on the other hand, increases difficulty of PM-cue retention and possibly requires participants to cycle through all PM cues in order to identify a PM target (A.-L. Cohen et al., 2008). Consequently, stress might primarily affect processing strategies in PM, but not PM maintenance and controlled search for PM cues in memory. This, however, remains speculative and future studies are required to further disentangle the effects of stress on different sub-components of PM.

While our findings suggest that stress targets monitoring processes under high demands, we can only speculate about the mechanisms through which these effects arise. First, a stress-induced improvement of selective attention (Chajut & Algom, 2003; Sänger et al., 2014) or an up-regulation of the salience network promoting cue-detection processes (Hermans, Henckens, Joëls, & Fernández, 2014) could have increased the efficiency of scanning stimuli for relevant and defining PM-cue features. While improvements in selective attention could explain reduced PM-monitoring costs, they also would have predicted both increased PM performance in focal and nonfocal conditions as well as reduced or no monitoring costs in the focal condition.

Second, under the assumption of spontaneous intention retrieval (at least to some small degree) in the nonfocal condition (McDaniel et al., 2015), increased engagement of habitual memory systems under stress (Schwabe, Joëls, et al., 2012; Schwabe & Wolf, 2009) could have shifted the participants’ processing strategy towards a greater reliance on spontaneous retrieval. As a consequence, less PM monitoring would be needed to detect PM cues. At the first glance, a shift towards spontaneous-retrieval-based PM performance might have also predicted increased PM performance in focal and nonfocal conditions as well as reduced or no monitoring costs in the focal condition, given that focal PM performance to a greater extend relies on spontaneous intention retrieval than more monitoring-based nonfocal PM performance (McDaniel et al., 2015; Scullin, McDaniel, Shelton, et al., 2010). However, research on instrumental learning of stimulus–response–

outcome associations showed that acute stress does not necessarily change encoding, acquisition and retrieval of stimulus–response links or stimulus–response–outcome associations, but alters strategies through which response selection occurs. For instance, while non-stressed individuals flexibly adjust response selection to a devaluation of previously learned outcomes, stressed individuals rigidly stick to previously learned stimulus–response associations and show no signs of adjustments to outcome devaluation (Schwabe & Wolf, 2009; for a demonstration of decreased cognitive flexibility in a selective attention task under acute stress, see also Plessow, Fischer, et al., 2011). Similarly, in the present study, acute stress might have affected participants' strategy of PM-cue detection, without affecting PM-cue-detection rates or intention retrieval prior to intention completion.

Interestingly, our finding of decreased monitoring costs in an event-based PM task suggest that it may be necessary to differentiate between acute stress effects on event-based and time-based PM. For instance, even though clock checking is considered an indicator of monitoring in time-based PM (e.g., Einstein & McDaniel, 1995), more frequent clock-checking under acute stress (Nater et al., 2006) might not necessarily reflect increased resource allocation towards the PM task (e.g., Hicks, Marsh, & Cook, 2005). Instead, this may be interpreted as a resource-saving strategy shift through which stressed individuals might have favored a conservative time-checking strategy over a presumably more resource demanding subjective time monitoring strategy.

4.5.3. Effects of Acute Stress on Intention Deactivation

The present study extended earlier findings of preserved intention deactivation (Walser et al., 2013) by showing that even under high demands on intention deactivation (i.e., with salient $\mathrm{PM}_{\mathrm{REPEATED}}$ cues in the focal condition) aftereffects of completed intentions in terms of performance costs on $\mathrm{PM}_{\mathrm{REPEATED}}$ compared to oddball trials were not affected by stress. This was true also for the nonfocal condition. At the same time, however, on a descriptive level commission errors were more prevalent in the focal condition for stressed than non-stressed participants. It should be noted though, that most participants did not make any commission errors at all. Those who did, however, only made a single one upon the first encounter of a $\mathrm{PM}_{\mathrm{REPEATED}}$ cue within a block. Still, 11 out of 40 stressed individuals compared to 3 out of 40 non-stressed individuals who committed a single commission error, suggest an initial susceptibility to habitual response activation under acute stress. While it seems surprising that stress did not affect RT aftereffects, this is most likely a result of participants utilizing feedback on erroneous PM responses. As a consequence, they rapidly adjusted to habitual response activation in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials, which could have attenuated stress effects on RTs and error rates in $\mathrm{PM}_{\mathrm{REPEATED}}$ compared to control trials. Importantly, however, given the lack of acute-stress effects on the inhibition of false-alarm PM responses prior to intention completion and the nominally reduced aftereffects of completed intentions for stressed compared to non-stressed individuals in the present study, it seems unlikely that commission errors increased due to generally impaired response inhibition resulting from a stress-induced PFC down regulation (Arnsten, 2015). Therefore, we assume that increased commission errors under acute stress reflect increased reliance on habitual memory systems (Schwabe, Joëls, et al., 2012; Schwabe & Wolf, 2009).

The effects of acute stress on PM and intention deactivation observed in this study were by no means global. Rather, under high task demands, acute stress produced isolated effects on monitoring costs and commission errors, while leaving PM performance unchanged. This suggests that acute stress effects on PM and intention deactivation are limited to specific processes in PM functioning and seem to depend on the resource requirements during PM-task performance and intention deactivation. Moreover, this presumed selectivity and demand dependence of acute-stress effects on PM can also explain null findings in previous studies with less challenging PM tasks (Nater et al., 2006; Walser et al., 2013; McLennan et al., 2016). Importantly, our findings corroborate assumptions of demand-dependent effects of stress on cognitive functions (e.g., Plessow et al., 2012; Shields et al., 2016) and support the notion that acute stress shifts processing strategies rather than generally impairs higher-order cognitive functions such as cognitive control.

4.5.4. Conclusion

When PM requirements are easy, acute stress experience does not change effort and/or resource allocation to the PM and other tasks at hand. However, when PM becomes demanding, acute stress results in changes of PM-monitoring processes that allow the individual to free up cognitive resources while maintaining a high PM performance level. Future studies are required to further disentangle the cognitive mechanisms underlying this shift in task performance toward higher efficiency and also address potential costs resulting from it (e.g., the increased likelihood of erroneously repeating completed actions, as observed in the present study).

5. Study 3 – Prospective Memory under Acute Stress: The Role of (Output) Monitoring and Ongoing-Task Demands

5.1. Abstract

Prospective memory (PM) refers to the ability to postpone retrieval and execution of intended actions until the appropriate situation (PM cue) has come, while engaging in other ongoing activities or tasks. In everyday life, we often perform PM tasks in stressful situations. While it has been shown that acute stress does not impair PM-cue identification and intention retrieval, little is known about acute stress effects on PM performance and memory for having performed an action (output monitoring) under varying ongoing-task demands. Here we investigated this in eighty healthy participants who performed event-based PM tasks during low- and high-demanding ongoing working memory tasks after having undergone either a standardized stress induction (Maastricht Acute Stress Test) or a standardized control protocol. Successful stress induction in the stress group compared to the no-stress group was confirmed by increased salivary cortisol, an indicator of stress-related hypothalamus-pituitary-adrenal axis activity, throughout the event-based PM tasks. Nevertheless, not-only PM-cue identification but also output monitoring remained fully intact after stress induction. The absence of these effects was independent of ongoing-task demands. Nonetheless, we replicated recent findings of a stress-induced reduction in performance cost of monitoring for PM-cue occurrences. Taken together, our findings suggest that acute stress alters PM monitoring by enhancing selective attention, decreasing PM response thresholds or by shifting performance towards more automatic processes in PM, while not affecting PM-cue identification and output monitoring.

5.2. Introduction

Our ability to postpone and remember actions for performance in the future is enabled by several cognitive functions that are tied together by the term prospective memory (PM; e.g., Kliegel, McDaniel, et al., 2008). PM enables us to perform simple tasks like passing a message to a colleague when we meet her in the office or picking up bread at a bakery on our way home. Simple PM tasks like these require us to delay intended actions while we perform other activities (ongoing tasks) and monitor the environment for opportunities to retrieve and perform these actions (PM cues). Additionally, more complex PM tasks can also require keeping track of whether (and when) we already performed an action—so-called output monitoring (A.-L. Cohen & Hicks, 2017b; Kliegel, Mackinlay, et al., 2008; Marsh, Hicks, et al., 2002; Meier & Zimmermann, 2015). For instance, when adhering to a medication schedule, we might need to monitor and remember whether we already took gastric protection before taking strong pain medication. Reports of rising levels of subjective stress in everyday life (e.g., S. Cohen et al., 2007; Lohmann-Haislah, 2012) suggest that we rely on PM and output monitoring increasingly under demanding conditions like acute stress. Yet, research on acute stress effects on PM is rare and its findings are ambiguous (Piefke & Glienke, 2017). Moreover, the effects of acute stress on output monitoring are unknown and it is also unclear in what ways ongoing-task demands affect PM functioning and output monitoring under stress. Therefore, in the present study we aimed at testing acute stress effects on PM with a focus on output monitoring in differently demanding ongoing tasks.

Acute stress that is often induced by experiences of uncontrollability and ego threat elicits a complex pattern of physiological responses (e.g., Charmandari et al., 2005; Dickerson & Kemeny, 2004; Joëls & Baram, 2009). Amongst the most prominently discussed physiological stress responses are the immediate release of catecholamines (i.e., noradrenaline, dopamine), often associated with a fast-paced activation of the sympathetic nervous system, the release of neuropeptides (e.g., corticotropin-releasing hormone), and the release of glucocorticoids (e.g., cortisol) into the bloodstream which is associated with a slower-paced increase of hypothalamus-pituitary-adrenal axis activity. Depending, for instance, on the type and duration of a stressor, these physiological stress responses act on different time scales and elicit a multitude of direct and interactive effects that have short and longer lasting effects (e.g., milliseconds to days; Joëls & Baram, 2009) on both the central and peripheral nervous system. Importantly, they alter neural activity and connectivity, for instance, in the prefrontal cortex (e.g., Arnsten, 2009, 2015; van Oort et al., 2017), the hippocampus (e.g., Kruse et al., 2018; Schwabe, Joëls, et al., 2012) and the visual cortex (e.g., Shackman et al., 2011; van Marle et al., 2009).

We argue that PM should be susceptible to acute stress effects for several reasons: (A) PM is associated with stress-sensitive prefrontal-cortex and hippocampus functioning, as evidenced by impaired event-based PM performance following frontal (and/or medial) brain injury or lesions (Carlesimo et al., 2014; Neulinger et al., 2016; Umeda et al., 2011; Uretzky & Gilboa, 2010; but see Volle et al., 2011; Kinch & McDonald, 2001; Cockburn, 1996 for preserved event-based but impaired time-based PM after brain injury). This is further corroborated by findings of impaired PM performance after repeated transcranial magnetic stimulation to dorsolateral prefrontal cortex (Bisiacchi, Cona, Schiff, & Basso, 2011) and suggested by neuroimaging studies (Beck et al., 2014; McDaniel et al., 2013; for an overview see Cona et al., 2016).

(B) Following theoretical models of PM (Kliegel, Altgassen, Hering, & Rose, 2011; Kliegel et al., 2002) as well as findings of impaired PM performance under high working-memory load (Kidder, Park, Hertzog, & Morrell, 1997; Marsh & Hicks, 1998; West & Bowry, 2005) and high demands on selective attention (e.g., during manipulations regarding divided attention; Clune-Ryberg et al., 2011; Harrison et al., 2014; Otani et al., 1997), we assume that PM requires working memory and selective attention that are known to be affected by acute stress (Shields et al., 2016; Weckesser, Alexander, Kirschbaum, Mennigen, & Miller, 2016).

Surprisingly, despite these putative pathways for stress effects on PM, several studies reported no effects of acute stress on PM performance in terms of our abilities to identify PM-cues and to retrieve intended actions. Particularly, in event-based PM tasks that required performing a simple action in response to a pre-specified PM cue, PM performance was not affected by acute stress (Möschl, Walser, Plessow, Goschke, & Fischer, 2017; Nater et al., 2006; Walser et al., 2013). PM performance in these tasks often relies on the encoding and retrieval of stimulus–response associations between a PM cue and an intended action (Cona et al., 2016; Einstein et al., 2005) that seem to be mostly preserved under stress (Schwabe, Tegenthoff, et al., 2012; Schwabe & Wolf, 2009). Hence one might assume that acute stress does not affect event-based PM performance at all. Importantly, however, PM consists of several sub-processes that enable actual performance of postponed actions. So far, it is mostly unclear how these sub processes are influenced by acute stress and under which conditions this influence occurs. For instance, the occurrence and extent of acute stress effects on PM might critically depend on specific task demands and might only affect certain sub-processes of PM.

Specifically, acute stress has been shown to seemingly improve PM functioning in rather demanding tasks that require self-initiated intention retrieval or high levels of PM monitoring. Glienke and Piefke (2016), for instance, observed that acute stress enabled stable event- and time-based PM performance in complex real-life simulations, while performance of non-stressed participants declined over testing. While they explained this finding by acute stress reducing or eliminating a primacy effect in PM (see also Glienke & Piefke, 2017), this effect might not be PM specific as acute stress-induced changes in primacy effects have also been observed, for instance, in retrospective memory (e.g., Vedhara, Hyde, Gilchrist, Tytherleigh, & Plummer, 2000). Similarly, Szőllősi et al. (2018) reported faster PM responses after acute stress induction in a demanding nonfocal event-based PM task, in which identifying PM cues (e.g., two even digits) required different processing than the ongoing task (e.g., comparing digits by size). Moreover, two studies found that acute stress might also alter stimulus processing or PM-monitoring strategies in demanding PM tasks. Nater et al. (2006) found that in a time-based PM task acute stress increased participants’ monitoring behavior in terms of more frequent clock checks and led to increased PM accuracy. Möschl et al. (2017) found that in a demanding nonfocal event-based PM task acute stress reduced costs in ongoing-task performance that are attributed to monitoring for PM cues (McDaniel et al., 2015; R. E. Smith, 2003; cf. Heathcote et al., 2015). More specifically, in their study stressed compared to non-stressed individuals showed a smaller difference between ongoing-task performance with as compared to without an additional PM task.

In summary, previous studies suggest that (a) acute stress effects on PM seem to mostly occur under high task demands and (b) acute stress does not always affect accuracy of PM-cue identification but may also have isolated effects on PM-response speed and PM monitoring. Importantly, however, the effects of acute stress on at least two additional components of PM-task performance have been neglected, namely output monitoring and the demands of ongoing tasks that are performed concurrently with a PM task.

First, since event-based PM tasks in previous stress studies only required a single response to rare PM cues, they provide no information about acute stress effects on output monitoring in PM. In PM research, output monitoring is typically assessed by having participants use different PM responses to indicate whether they remembered having already responded to a PM cue. Here, failures of output monitoring manifest, for instance, when participants repeat the same response as during their first encounter of a PM cue (Einstein, McDaniel, Smith, & Shaw, 1998; Marsh et al., 2007; Marsh, Hicks, et al., 2002; Meier & Zimmermann, 2015; Skladzien, 2010). Repetition errors like these have been attributed to errors of source- or reality monitoring in that participants classify performed actions as planned or imagined actions (Johnson, Hashtroudi, & Lindsay, 1993; Lindsay & Johnson, 1991; McDaniel, Lyle, Butler, & Dornburg, 2008) or to participants not remembering their previous action (Arnold & Lindsay, 2005; Koriat et al., 1988; Marsh, Hicks, et al., 2002; Meeks, Hicks, & Marsh, 2007). The latter in particular may be viewed as a failure of episodic memory (for an overview, see Tulving, 2002; Wheeler, Stuss, & Tulving, 1997) in that repetition errors occur due to difficulties in retrieving or a lack of episodic traces of past performance. While to our knowledge there is only correlational evidence for a connection between output monitoring in PM and episodic memory (Ball, Pitães, & Brewer, 2018), we think it is feasible that this connection to stress-sensitive episodic memory (Shields et al., 2017) provides a potential pathway through which acute stress could affect output monitoring.

Second, previous studies that reported acute stress effects on PM mostly induced high demands on PM-cue identification, intention retrieval or PM monitoring (Piefke & Glienke, 2017). However, little is known about the role of ongoing-task demands for PM functioning under stress. PM and ongoing-task performance are thought to rely on shared resources (Einstein & McDaniel, 2005; Scullin et al., 2013; R. E. Smith, 2003) that are distributed according to their respective demands or meta-control beliefs (e.g., Rummel et al., 2017). Accordingly, ongoing tasks have been shown to impair PM performance when they require high demands on working memory (Bisiacchi, Tarantino, & Ciccola, 2008; Kidder et al., 1997; R. E. Smith & Bayen, 2005), spatial attention (Marsh, Hancock, & Hicks, 2002) or response selection (Meier & Zimmermann, 2015). Given the demand dependence of acute stress effects on PM, it is feasible that acute stress may affect PM when resources for PM performance are limited due to a demanding ongoing task.

5.3. The Present Study

In light of the apparent robustness of event-based PM performance under stress, one might assume that acute stress has no (negative) effects on event-based PM performance. This conclusion, however, would be premature, given our limited understanding of acute stress effects on the conglomerate of subprocesses that make up PM. Therefore, our main aims in the present study were to test the effects of an acute stress experience on PM-cue identification, output monitoring, and PM monitoring under varying ongoing-task demands in order to help identify conditions under which PM may be susceptible to acute-stress effects. Regarding the direction of stress effects on PM, we formulated two hypotheses.

Hypothesis 1 builds on the notion that acute stress impairs prefrontal-cortex related higher-order cognitive functions (Arnsten, 2009, 2015; Shields et al., 2016) and hippocampus related episodic-memory encoding and retrieval (Shields et al., 2017). Accordingly, acute stress should impair PM components that rely on or are associated with these brain areas and cognitive functions, such as PM-cue identification, intention retrieval, output monitoring, and PM monitoring (Cona et al., 2015). Specifically, acute stress induction should lead to slower and more erroneous PM-cue identification and output monitoring (more repetition and counting errors) as well as higher PM-monitoring costs.

Hypothesis 2 builds on the notion that acute stress has diverging effects on different memory systems and cognitive processes. On the one hand, acute stress has been shown to enhance encoding and retrieval of stimulus–response links (Schwabe, Joëls, et al., 2012; Schwabe & Wolf, 2009), which should increase speed and accuracy of PM-cue identification and may reduce PM monitoring costs through shifting PM performance more towards spontaneous-retrieval based processes (Möschl et al., 2017). On the other hand, acute stress has been shown to impair encoding and retrieval of working memory (Schoofs et al., 2008, 2009) and episodic memory (Shields et al., 2017) that can be related to output monitoring. Accordingly, acute stress induction should lead to slower output-monitoring responses as well as more repetition- and counting errors. Note that acute stress could also affect PM via alterations of selective attention. However, here predictions are less clear. While several studies showed that acute stress impairs selective attention for neutral stimuli compared to stressor-related or negatively valenced stimuli (e.g., Herten, Otto, & Wolf, 2017; Schwabe & Wolf, 2010) or in stressor-related tasks (Chajut & Algom, 2003), others showed improved selective attention for task-relevant neutral stimuli under acute stress (Sato, Takenaka, & Kawahara, 2012; Tiferet-Dweck et al., 2016; c.f. Braunstein-bercovitz, 2003; Sänger et al., 2014) and yet others showed the opposite. Assuming that the stimuli in the present study are neutral (i.e., not stress related), enhanced selective attention for task-relevant stimuli should increase speed and accuracy of PM-cue identification and reduce PM-monitoring costs, while impaired selective attention should impair PM-cue identification and increase PM-monitoring costs.

Regarding the effects of ongoing-task demands on PM functioning under acute stress, we reasoned that high ongoing working-memory demands would reduce cognitive resources for PM performance and increase sensitivity of PM towards acute stress effects. Consequently, we expected that stress-induced performance benefits should be attenuated, while stress-induced performance costs should be increased under high compared to low ongoing-task demands. To test these hypotheses, we assessed PM-cue identification, output-monitoring and PM monitoring in an event-based PM task that instructed participants to also monitor for the number of PM-cue encounters during either low-demanding 1-back and high demanding 2-back working-memory tasks (Figure 12).

Additionally, since only few studies investigated acute stress effects on the deactivation of completed intentions (Möschl et al., 2017; Walser et al., 2013), we also aimed to explore this in the present study. For this we presented no-more-relevant PM cues from finished PM tasks as $\mathrm{PM}_{\mathrm{REPEATED}}$ cues during subsequent ongoing-task blocks and assessed whether participants continued to give PM responses (so-called commission errors)(Scullin et al., 2012; Walser et al., 2012, 2017). Note that in previous studies, acute stress only affected intention deactivation when $\mathrm{PM}_{\mathrm{REPEATED}}$ cues were difficult to ignore. Specifically, acute stress nominally increased participants risk of making a commission error when $\mathrm{PM}_{\mathrm{REPEATED}}$ cues were salient and focal, but not when PM cues were nonfocal (Möschl et al., 2017). Similarly, Walser et al. (2013) observed overall only few commission errors when PM cues were focal but nonsalient and no effects of acute stress on ongoing-task interference in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials. In the present study, we also chose nonfocal and nonsalient PM cues in order to induce high demands on PM-cue identification and PM monitoring. Consequently, we did not expect many commission errors (Scullin et al., 2012) and cannot make clear predictions about acute stress effects on intention deactivation. Hence, we assessed commission errors purely for exploratory reasons. This is nonetheless important, as previous studies might have underestimated acute stress effects on the commission error risk: Erroneous PM responses always triggered auditory error feedback which likely fostered rapid response reconfiguration after PM-task completion (Möschl et al., 2017). To avoid this, in the present study we did not provide any error feedback.

After baseline performance of the event-based PM task, participants were randomly assigned to either a stress or a no-stress treatment (Figure 13). Participants in the stress treatment performed the Maastricht Acute Stress Test (MAST; Smeets et al., 2012), which combines elements of physiological and psychological stress induction by having participants repeatedly immerse their hand into ice-cold water and perform mental arithmetic in the presence of an experimenter while being filmed. Participants in the no-stress treatment performed hand immersions in warm water and simple mental arithmetic without videotaping.

5.4. Methods

5.4.1. Participants

Eighty students of the Technische Universität Dresden (40 male; 18 – 30 years, *M* = 21.79 years, *SD* = 3.16 years; 72 right-handed, 2 without preference) participated in a single 2-hour long experimental session in exchange for 16 € or course credits. Experimental sessions took place between 10:00 am and 8:00 pm. The distributions of testing time did not differ significantly between stress (*Mdn* = 2:19 pm) and no-stress groups[12] (*Mdn* = 2:00 pm), Mann–Whitney *U* = 693.00, *p* = .303. Hence, we assume that potential stress effects cannot be explained solely by group-differences in circadian variability of PM performance (Rothen & Meier, 2017). In order to reduce inter-individual variability of stress reactivity, we only tested participants who were healthy, medication free and of normal weight (body mass index between 18 and 27, *M* = 22.40, *SD* = 2.30 kg/m^2) (Kudielka et al., 2009). Given that reduced physiological stress responses were reported for habitual smokers (Rohleder & Kirschbaum, 2006) and oral-contraceptive intake (Kirschbaum et al., 1999), participants were non-smokers (i.e., less than 5 cigarettes per week) and all female participants reported

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[12] Following suggestions from an anonymous reviewer, we conducted *t*-tests and Levene tests of group differences in testing time (time of day converted to hours) on the whole sample and separate for male and female participants. Whole-sample tests did not suggest substantial differences in mean testing time between treatment groups (stress: 13.74 h, no-stress: 14.29 h): *t*(78) = −1.08, *p* = .281, *d* = 0.24, but slightly larger variance of testing time in the no-stress group (6.04 h) than in the stress group (4.16 h), *F*(1, 78) = 3.69, *p* = .061. Separated tests did not suggest substantial differences in testing-time distributions between the stress and no-stress group for either female participants, *t*(38) = −1.54, *p* = .131, *d* = 0.49, *F*(1, 38) = 1.37, *p* = .250; or male participants, *t*(38) = 0.007, *p* = .995, *d* = 0.002, *F*(1, 38) = 1.56, *p* = .220.

to refrain from using hormone-based birth control. All participants had normal or corrected-to-normal vision.

5.4.2. Ethics Statement

All procedures were in accordance with the ethical standards of the institutional review board for performing studies involving human participants at the TU Dresden (OHRP-numbers: IRB00001473, IORG0001076; case number: EK415092015) and with the 1964 Helsinki declaration and its later amendments or comparable ethical standards. Informed consent was obtained from all individual participants before performing any study protocols.

5.4.3. Stress Induction and Stress Validation

Participants were randomly assigned to a stress (20 male, 20 female) or a no-stress control treatment (20 male, 20 female). The stress treatment was the MAST (Smeets et al., 2012), which started with a 5 min instruction and anticipation phase, followed by a 10 min stress-induction phase. During stress induction, participants alternated several times between immersing their dominant hand up to the wrist in ice-cold water for 60–90 s (water temperature in the present study: $M = 2.04$, $SD = 0.25$ °C) and performing mental arithmetic tasks for 45–90 s. While performing these tasks, participants received video feedback of their facial expressions on a monitor in front of them. Additionally, an experimenter sitting behind the participant supervised the participant's performance. The no-stress control treatment matched the procedure of the MAST, but included warm water (water temperature in the present study: $M = 36.99$, $SD = 0.10$ °C), only simple mental arithmetic and lacked videotaping and -feedback (Smeets et al., 2012).

We assessed biological treatment consequences via levels of salivary α-amylase (sAA) and salivary cortisol as markers of sympathetic-nervous-system activity (Thoma et al., 2012; cf. Bosch et al., 2011) and hypothalamic-pituitary-adrenal-axis activity (Hellhammer et al., 2009; Kirschbaum & Hellhammer, 1994), respectively. For this, we collected saliva samples via Salivette® sampling devices (blue cap; Sarstedt, Nümbrecht, Germany) 1 min before treatment start and 1, 10, 30 and 50 min after treatment, respectively (Figure 13). Concentrations of sAA and cortisol were analyzed with quantitative enzyme-kinetic methods (reagents nr. 11876473316 & 10759350-190, Roche Diagnostics Deutschland GmbH; sensitivity: 2 U/ml) (Rohleder & Nater, 2009) and chemiluminescence immunoassays (CLIA, IBL International, Hamburg, Germany; sensitivity: 0.2 nmol/l). Intraand inter-assay variabilities were less than 8%. In order to reduce the risk of a systematic bias in the analyses, participants from the stress and no-stress treatments were distributed randomly across plates[13]. To assess subjective treatment consequences, participants completed the German Mehrdimensionaler Befindlichkeitsfragebogen (MDBF; English version: Multidimensional mental-state questionnaire; Steyer et al., 1997) simultaneously to the saliva samples.

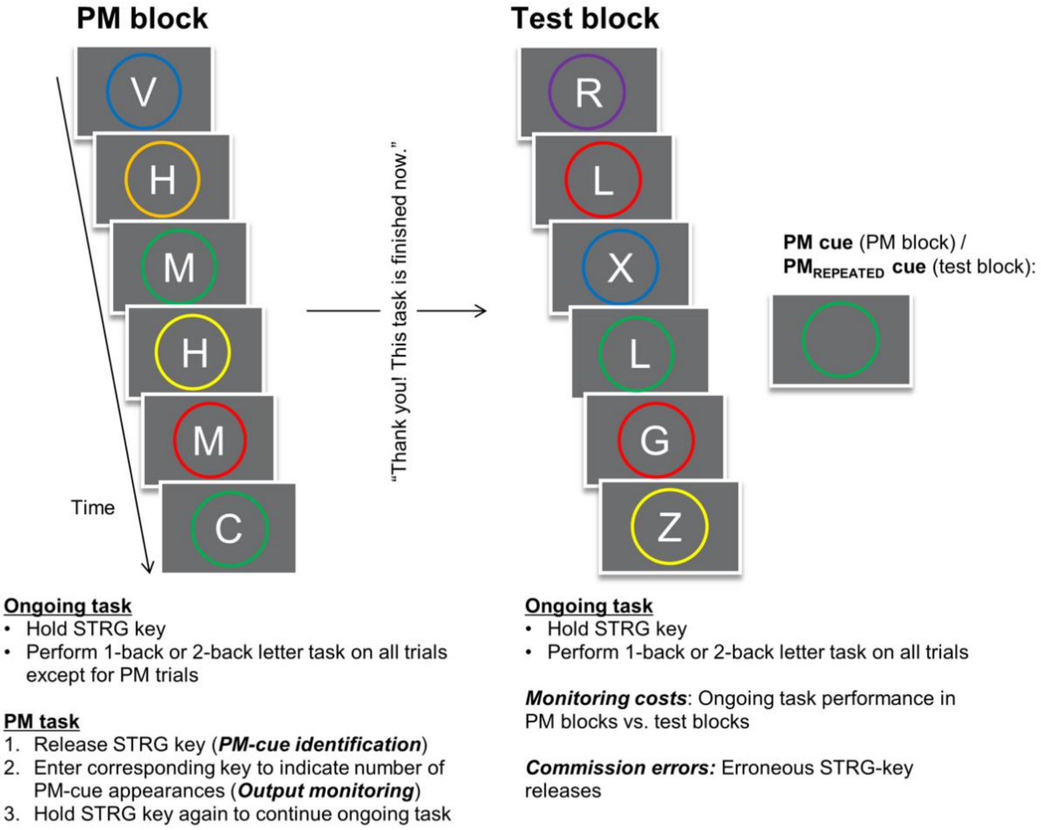

Figure 12. Cognitive task. Example trial sequences during PM and test blocks. As ongoing task, participants decided whether the target letter matched (press comma key with right index finger) or mismatched (press period key with right middle finger) the letter in the trial before (1-back task) or two trials before (2-back task). Additionally, they had to press and hold the STRG (CTRL) key in order to keep the experiment running. In trials with PM cues (e.g., green circle), participants had to release the STRG key (PM-cue identification measure) instead of performing the ongoing *n*-back task and subsequently indicate the number of PM-cue encounters within the PM block (output-monitoring measure). In test blocks, participants exclusively had to perform the ongoing *n*-back task. Nevertheless, in some trials colored circles, which served as PM cues in the PM block, were shown as no-more-relevant PM cues (i.e., PM$_{\text{REPEATED}}$) to measure participants' risk of making commission errors after intention completion. PM-monitoring costs were assessed as ongoing-task performance differences between the PM and test blocks.

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[13] To achieve randomized distribution of participants across analysis plates, each participant was assigned a random number between 1 and 80 and was assigned randomly to the stress or no-stress treatment. After completion of data collection, saliva samples for all participants were numbered consecutively in an ascending order and distributed to plates accordingly.

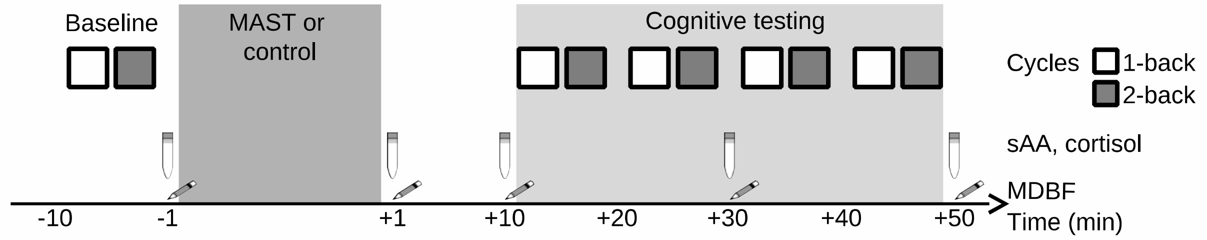

Figure 13. Procedure. Illustration of the procedure with baseline assessment of the cognitive task, treatment (i.e., Maastricht Acute Stress Test [MAST; Smeets et al., 2012] vs. control), cognitive testing and measurement time points of saliva samples (salivary α-amylase [sAA], cortisol) and the German MDBF questionnaire (Mehrdimensionaler Befindlichkeitsfragebogen; English version: Steyer et al., 1997). White and grey squares illustrate experimental cycles that consisted of a PM block and test block. At the beginning of the experimental session, participants briefly practiced the ongoing 1-back and 2-back tasks (not depicted). During subsequent baseline assessment, they completed one 1-back and one 2-back cycle of the cognitive task. After treatment, the cognitive task comprised eight cycles. The 1-back and 2-back cycles alternated throughout the experiment. Half of the participants started with a 1-back task (as depicted here), whereas the other half started with a 2-back task.

5.4.4. Cognitive Task

To test our hypotheses, we assessed PM-cue identification, output-monitoring and PM monitoring in an event-based PM task with features that in previous studies proved suitable to detect potential acute stress effects on PM (Figure 12). First, we induced high demands on PM-cue identification and PM monitoring by using nonsalient nonfocal PM cues (see also Möschl et al., 2017; Szőllősi et al., 2018) in an ongoing *n*-back working-memory task. Second, we manipulated ongoing-task demands by alternating low-demanding 1-back and high demanding 2-back tasks (Figure 13). This choice was based on findings of impaired PM performance under additional working-memory demands (Bisiacchi et al., 2008; Kidder et al., 1997; Marsh, Hancock, et al., 2002; R. E. Smith & Bayen, 2005) and the shared association of anterior prefrontal cortex functioning to PM-performance in nonfocal PM tasks (Cona et al., 2016) and *n*-back performance (Schmidt et al., 2009). This provides a potential pathway through which ongoing *n*-back demands might affect acute stress effects on PM. Third, to assess PM-cue identification and output monitoring, participants were instructed to release a specific button in response to PM cues and subsequently indicate how many PM cues they had encountered during the task block (for similar approaches, see Marsh et al., 2007; Marsh, Hicks, et al., 2002; Meier & Zimmermann, 2015). This design allowed us to assess the quality of output monitoring via the number of repetition errors and the number of errors in participants' subjective counting of PM-cue encounters (i.e., subjective counting errors).

Target stimuli for the cognitive task were capital letters (B, C, F, G, H, J, L, M, R, V, X, Z, Arial font, size: 20 points; visual angle ≈ 1.3° in a viewing distance of 60 cm) surrounded by circles in 10 different colors (inner diameter: 202 pixels, outer diameter: 212 pixels, visual angle ≈ 6.0°; see Appendix C, Table C1 for color values). Stimuli were centrally displayed on a 19-inch TFT monitor (resolution 1280 × 1024 pixels), using a Windows XP SP3 personal computer with a QWERTZ keyboard running Presentation® software (Version 16.3; www.neurobs.com).

In the ongoing *n*-back tasks, participants were instructed to indicate whether the current letter matched the letter in the preceding trial (1-back task) or the letter that was presented two trials before (2-back task). Participants pressed the *comma* key with right index finger to indicate an *n*-back match and the *period* key with right middle finger to indicate an *n*-back mismatch. For both *n*-back tasks, 1/3 of trials were match trials and 2/3 were mismatch trials.

With the exception of short breaks and PM trials, participants were required to press and hold the *STRG* (*CTRL*) key with their left index finger in order to keep the experiment running. In PM trials, participants were instructed to release their left index finger from the STRG key and subsequently indicate the number of PM cues that they had encountered within a block by pressing the corresponding number key on the upper row of the keyboard in an input dialog. To this end, after releasing the STRG key, an input dialog appeared that displayed the instruction “*Please make your input.”* above a white square (height: 100 pixels, visual angle ≈ 2.8°) in the center of the screen. Upon participants’ first response on the number keys 1 to 9 the corresponding digit was presented in black on the square (Arial font, size: 48 points, visual angle ≈ 3.1°) and remained until participants continued the experiment by holding STRG.

Each trial had a fixed duration of 2500 ms and comprised a target letter surrounded by a colored circle that appeared simultaneously with the letter. While the target letter disappeared after 500 ms, circles remained on the screen for the entire duration of the trial. The target letter and the circle color changed from trial to trial. No fixation cross, no inter-trial interval and no error feedback at the end of a trial were presented. Consequently, participants constantly saw a circle on the screen that changed color from trial to trial. Target letters and circle colors in the *n*-back tasks were assigned randomly to trials with the constraint that the same color did not occur on two subsequent trials.

The experimental session started with a brief practice of the 1-back task followed by the 2-back task for 40 trials each (all 10 circle colors were presented 4 times in random order). During practice, participants were instructed that they needed to press and hold the

STRG key throughout the experiment in order to keep it running. Releasing the STRG key interrupted the ongoing task and prompted the instructions "*Attention: You took your finger off the STRG button.*" at the top and "*Hold STRG to continue.*" at the bottom of the screen.

After task practice, participants performed ten experimental cycles each consisting of a PM block and a test block. While PM blocks served to assess PM performance and output monitoring, test blocks served as a no-intention control condition to assess monitoring costs and to measure commission errors after PM-task completion. In order to assess participants baseline performance in the cognitive task, they performed two PM-block–test-block cycles prior to the stress/no-stress treatment (see Möschl et al., 2017; Walser et al., 2013 for a similar design). The cognitive testing after treatment consisted of eight PM-block–test-block cycles. In order to aid collection of saliva samples, participants were prompted to notify the experimenter ("*Please notify the experimenter.*") after cycles 2, 6, and 10. Additionally, we included short breaks ("*Short break. Please wait until the experiment continues by itself.*", 20 s) after cycles 4 and 8.

In PM blocks, participants were instructed to perform an *n*-back task and a PM task that required giving a different response than performing the ongoing task. More specifically, whenever they encountered circles of a specific color (e.g., green), they were to release the STRG key and type in how often the PM cue had already appeared within the current PM block (e.g., first appearance press the "1" key, second appearance press the "2" key). Participants were also presented with an exemplar of the PM cue during instructions. These instructions were displayed in written form on the screen at the beginning of the block and remained until participants started the block by pressing the STRG key. Releases of the STRG key served to measure the correct identification of a PM cue, whereas the indication of the number of PM cue appearances served to assess output monitoring. To continue with the ongoing task, participants had to press and hold the STRG key again.

In test blocks, participants were instructed to exclusively perform the ongoing task, but were still required to hold the STRG key during task performance. No-more-relevant PM cues from the PM block were re-presented as $\text{PM}_{\text{REPEATED}}$ trials to assess commission errors after intention completion (i.e., STRG-key releases). Each block ended with the information "*Thank you. This task is finished now.*" together with feedback concerning mean response times (RT) and the number of errors made in the block (8 s).

Each PM block consisted of 64 trials. During baseline assessment, PM blocks contained 4 PM trials. In order to reduce predictability of the number of PM-cue encounters, after treatment the quantity of PM trials varied between cycles: Both the first (cycles 3–6) and the second half of the experiment after treatment (cycles 7–10) consisted of two cycles with 4 PM trials, one cycle with 3 PM trials and one cycle with 5 PM trials. It was determined randomly which cycles would contain 3, 4, or 5 PM trials within the experiment halves. In order to keep the length of PM blocks constant between cycles, the number and color of standard trials were adjusted according to the PM-trial quantity in a block. That is, PM blocks with 4 PM trials contained 60 ongoing-task trials (i.e., non-PM trials) in 6 different colors (10 trials per color). In blocks with 3 PM trials, one of the 6 ongoing-task-trial colors was randomly chosen and presented 11 times. In blocks with 5 PM trials, one of the 6 ongoing-task-trial colors was randomly chosen and presented 9 times. Each test block consisted of 32 trials. Trials comprised 2 $\mathrm{PM}_{\mathrm{REPEATED}}$ trials with the same colored circle as PM trials in the preceding PM block and 30 ongoing-task trials of the 6 colors used in the PM block, each presented 5 times.

Each color served as a PM/$\mathrm{PM}_{\mathrm{REPEATED}}$ cue in only one cycle of the experiment. To prevent carry-over effects from finished PM tasks to novel PM tasks, the color that served as a PM/$\mathrm{PM}_{\mathrm{REPEATED}}$ cue in a given cycle did not appear in any of the subsequent three cycles but was substituted with one of the remaining colors. Distribution of PM/$\mathrm{PM}_{\mathrm{REPEATED}}$ trials throughout blocks was random with at least one ongoing-task trial in between two subsequent PM/$\mathrm{PM}_{\mathrm{REPEATED}}$ trials. PM and $\mathrm{PM}_{\mathrm{REPEATED}}$ cues were assigned randomly to appear in *n*-back match or mismatch ongoing-task trials.

5.4.5. Procedure

In order to reduce variations in blood-glucose levels, participants were instructed to refrain from eating and drinking sugar-based drinks for two hours before the experimental session. At session start, written informed consent was obtained and basic demographic information was assessed. After that, all participants received 200 ml grape juice to attempt to standardize inter-individual blood-glucose levels, the availability of which is a prerequisite for the stress-induced increase of hypothalamic-pituitary-adrenal-axis activity (Kirschbaum et al., 1997). Participants subsequently performed the baseline assessment in the cognitive task before they underwent the stress or no-stress treatment at 45 min after the beginning of the experimental session. From 10 min to 50 min after treatment cessation, the cognitive task was administered. Delaying the cognitive testing for 10 min allowed us to capture performance under peak-cortisol levels between 10–30 min after treatment (e.g., Engert et al., 2011; Kirschbaum et al., 1997), while stressed and non-stressed individuals should show comparable sympathetic nervous system activity and subjective mood levels.

5.4.6. Data Preparation and Trimming

Due to technical errors with the response logging, we excluded 4.3% of trials from the analysis[14]. Except for a nominally smaller stress-induced reduction of PM-monitoring costs in ongoing-task RTs under high compared to low ongoing-task demands, results did not change when using the complete dataset (Appendix C, Table C3).

Analyses of PM-cue identification, output monitoring and PM-monitoring costs were performed on (natural) log-transformed RTs for correct responses and on error rates (Table 10, Figure 15). PM-cue identification was classified as correct, if participants released the STRG key in a PM trial. Output monitoring was classified as correct if participants correctly identified a PM cue and indicated either the objectively correct number of PM-cue encounters (i.e., objective accuracy) or the number of PM-cue encounters in the present PM block according to their subjective counting. Hence, correct output-monitoring responses also include instances in which participants missed one or several PM cues or might have made false-alarm PM responses in standard trials before encountering a PM cue. For instance, if participants missed the first PM cue in a block and counted the following PM-cue encounters as *1, 2,* and *3* or if they responded with *1, 3,* and *5* to PM-cue encounters. Output-monitoring errors were trials in which participants gave the same PM-cue counting response as in the preceding PM trial (repetition error) and trials in which participants gave a PM-cue counting response lower than their counting response in the preceding PM trial (i.e., subjective counting errors). Rare cases, in which repetition errors or subjective counting errors indicated an objectively correct counting response, were classified as output-monitoring errors (5 repetition errors, 2 subjective counting errors).

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[14] Initial visual inspection of the data revealed that some participants unexpectedly produced missing data in PM and $\text{PM}_{\text{REPEATED}}$ trials. Although response data in these trials suggests that they gave a PM response, due to a technical error response data for the output-monitoring response is missing (i.e., no counting response and no counting RT). Overall this type of missing response data occurred in 93 trials (standard: 17, PM: 41, $\text{PM}_{\text{REPEATED}}$: 35 trials), with one participant in the stress group producing 40 missing data points. We reasoned that including this missing response data in our analyses would artificially deflate output-monitoring errors and inflate the commission-error risk after intention completion. Consequently, we excluded data from cycles with at least one missing data point in a PM or $\text{PM}_{\text{REPEATED}}$ trial. This amounted to excluding one subject in the stress group, who had missing data in each cycle of the experiment, 8 cycles (across 7 participants) in the control group and 16 cycles (across 7 participants) in the stress group. Overall, excluding 1 participant and 24 cycles left 40 participants (37632 trials) in the control group and 39 participants (35904 trials) in the stress group.

For the RT analyses of PM performance, we excluded errors (PM-cue detection: 28.2% of PM trials, output monitoring: 5.2% of detected PM trials). We then excluded trials with log-transformed RTs above or below 2.5 *SD*s from the mean log-transformed RT for correct PM-cue identification (1.1%) and correct output-monitoring responses (2.1%).

In accordance with data-preparation recommendations for the analysis of PM-monitoring costs in ongoing-task performance (Brewer, 2011), we excluded PM and $\text{PM}_{\text{REPEATED}}$ trials (6.3%) as well as two trials following a PM trial (5.9%) to avoid overestimating monitoring costs due to aftereffects of PM cues (Meier & Rey-Mermet, 2012). For the RT analysis of monitoring costs, we subsequently excluded error trials (0.8%), response omissions (0.01 %) and outlier trials with log-transformed RTs above or below 2.5 SDs from the mean log-transformed RT for correct ongoing-task performance (2.0%).

5.4.7. Statistical Analyses

The conventional two-stage analyses of between-subject designs in cognitive psychology entail a substantial loss of statistical power because all trial-specific information is lost due to data aggregation at the individual level (Raudenbush, 1997). Therefore, we analyzed the collected data within a mixed-effects generalized linear modeling framework that allowed us to estimate the effects of interest while accounting for trial-level and residual individual variability in task performance.

Model specifications. We analyzed RTs with a log-normal regression model (see also Ulrich & Miller, 1993) of the form:

$$log\left(RT_{i,n}\right) = X\mathrm{B} + \xi_i + \varepsilon_{i,n}$$

Here $RT_{i,n}$ denotes the conditional geometric mean RT of the i^{th} individual in the n^{th} trial and $X\mathrm{B}$ represents the fixed linear predictor (i.e., the intercept, baseline covariates, and treatment contrasts) for an individual given his/her response characteristics (e.g., variability in RT) and other contrasts of interest (e.g., assignment to the stress group). Since we only analyzed RTs trials with correct responses, we included $\xi_i \sim Normal(0, \theta)$ to account for the random deviation of the i^{th} individual from the mean RT. We also added $\varepsilon_{i,n} \sim Normal(0,\ \sigma)$ to account for the residual deviation of the log-transformed RTs from their conditional mean. In accordance with this model specification, e^{B} can be interpreted as the relative change of the mean RT given the characteristics of the individual (i.e., the intercept for a subject) and other contrasts of interest. For instance, this could represent the relative change of the mean RT in the control group when participants were exposed to the stress treatment.

For the analysis of error probabilities (ERR), we used a binomial regression model of the following form:

$$logit\left(ERR_{i,n}\right) = X\mathrm{B} + \xi_i$$

Here $ERR_{i,n}$ denotes the probability of the ith individual to commit an error in the nth trial, $X\beta$ represents the fixed linear predictor of the conditional error probability, and $\xi_i \sim Normal(0, \theta)$ represents the random deviation of the ith individual from the mean error probability. Using this model specification, e^{B} can be interpreted as the odds ratio (OR) for committing an error given the characteristics of the individual and other contrasts of interest, e.g., the odds ratio to make an error when participants were exposed to the stress compared to the control treatment.

Baseline covariates and predictors. In order to reduce unspecific variance in the performance estimates, we included factors that may affect task performance or stress effects on cognition as baseline covariates with additive terms in all models: First, we included the covariate cycle (1–10) to account for time-dependent effects of stress on cognitive functions (Hermans et al., 2014; Plessow, Fischer, et al., 2011; Schwabe, Joëls, et al., 2012). Second, we included the variable *n*-back-target type (match vs. mismatch) to account for a potential performance bias. Specifically, due to the low number of *n*-back-match compared to *n*-back-mismatch trials in our experiment, ongoing-task and PM performance in *n*-back-match trials might be impaired. Third, we included the number of PM cues per block (3–5) to account for its potential effects on PM performance, output monitoring and ongoing-task performance, as have been observed, for instance, in PM-monitoring costs (A.-L. Cohen et al., 2008).

Furthermore, to foster interpretability of the baseline condition in each model (i.e., the intercept), we centered the baseline covariates to meaningful data points after data preparation but prior to model fitting. Consequently, estimates based on the intercept represent performance by the no-stress group in PM blocks at averaged levels of *n*-back-target type and ongoing-task demand with 4 PM cues per block at approximately 30 min after treatment.

Lastly, to account for individual variability in (baseline) task performance, we conducted all analyses on data from cycles both prior to and after treatment. Additionally, to account for individual variability in baseline stress levels and individual stress responses, all participants were coded as belonging to the control condition in cycles 1–2 prior to treatment, so that the factor treatment differed only during cycles 3–10 after treatment.

All models were fitted using the lme4 package (version 1.1-15; Bates, Mächler, Bolker, & Walker, 2015) with R 3.4.3 statistical software (R Core Team, 2017). For RT analyses, *p*-values, 95% confidence intervals for fixed-effect coefficients and model predictions were approximated based on Satterthwaite-corrected degrees of freedom (Satterthwaite, 1941) with the lmerTest package (version 2.0-36; Luke, 2017), stats (version 3.4.3) and emmeans packages (version 1.1; Lenth, Lenth, & Matrix, 2015), respectively. For error analyses, these parameters were approximated using infinite denominator degrees of freedom and asymptotic Wald statistics. The Bayes factor for the alternative hypothesis (BF_{10}) of performed hypothesis tests (odds of H_1 to H_0) for which $p < 1/e$ ($p < .368$) was estimated from *p*-value estimates by calculating the inverse of the $BF_{01,}$ as was outlined by Sellke, Bayarri and Berger (2001, Formula 2). The R script and the data used for the reported analyses can be downloaded from https://osf.io/pcnfg.

5.5. Results

5.5.1. Preliminary Analysis of the Cognitive Task

Before examining acute stress effects on PM functioning, we assessed whether performing a PM task with nonfocal PM cues produced PM-monitoring costs in ongoing-task performance (e.g., McDaniel et al., 2015) and whether increasing ongoing-task demand (1-back vs. 2-back) impaired PM-cue identification and output monitoring, and increased PM-monitoring costs in RTs and error rates (e.g., Meier & Zimmermann, 2015). Note that since these tests are not part of our main analyses, we cannot ensure appropriate control for type-I-error inflation in these tests. Therefore, here *p*-values are reported solely to allow estimating the degree of evidence for the H_0, given the data.

Slowing of ongoing-task RTs by 14% during PM blocks compared to test blocks, e^B = 1.14, 95% CI [0.38, 1.45], $p < .001$, $BF_{10} > 150$, and an increase in error risk by 101% in ongoing-task performance during PM blocks compared to test blocks, *OR* = 2.01, 95% CI [1.77, 2.29], $p < .001$, $BF_{10} > 150$, suggested the presence of PM-monitoring costs in ongoing-task performance. As expected, ongoing-task demand affected PM-cue identification, producing slowed PM responses under high demands by 11%, e^B = 1.11, 95% CI [1.07, 1.16], $p < .001$, $BF_{10} > 150$, and an increase in error odds by 113%, *OR* = 2.13, 95% CI [1.71, 2.65], $p < .001$, $BF_{10} > 150$. By contrast, high ongoing-task demands only slightly slowed output-monitoring responses by 5%, $e^B = 1.05$, 95% CI [1.00, 1.10], $p = .050$, $BF_{10} = 2.460$, and did not affect odds for repetition and subjective-counting errors in output monitoring, $OR = 0.84$, 95% CI [0.48, 1.46], $p = .536$. Surprisingly, increasing ongoing-task demands reduced monitoring costs by 21% in RTs, $e^B = 0.96$, 95% CI [0.95, 0.97], $p < .001$, $BF_{10} > 150$, and by 3% in error odds $OR = 0.70$, 95% CI [0.60, 0.83], $p < .001$, $BF_{10} > 150$.

5.5.2. Main Analysis

To account for multiple testing in our main analysis of acute stress effects on PM functioning, we used a combination of gatekeeping hypotheses and Bonferroni-adjusted *p*-value thresholds (Dmitrienko, Tamhane, & Wiens, 2008). More specifically, using this gatekeeping procedure we conducted analyses on three levels, starting with a *p*-value threshold of $\alpha = .05$. On the first level of analysis, we conducted treatment checks to assess if treatment induced physiological and psychological stress responses. This entailed testing for treatment effects on salivary cortisol, salivary α-amylase, and the MDBF scales *good mood vs. bad mood*, *calmness vs. restlessness*, and *alertness vs. fatigue* at a Bonferroni-corrected *p*-value threshold of $\alpha = .01$ [.05/5] for each test. On the second level of analysis, we assessed treatment effects on PM-cue identification, output monitoring, and PM-monitoring costs in RTs and error rates, resulting in six different tests. The *p*-value threshold for each of these tests was constructed by dividing the sum of the *p*-value thresholds from significant first-level tests by six. On the third level of analysis, we tested whether any significant second-level treatment effect was modulated by ongoing-task demands. The *p*-value thresholds for these tests were recycled from significant second-level tests. The point estimates and standard errors of the untransformed regression coefficients from the main analyses are listed in Table 9. Information about the random effects characteristics for the main analyses is listed in Appendix C, Table C2.

Treatment checks. We conducted all analyses of physiological and psychological treatment effects at a Bonferroni-corrected *p*-value threshold of $\alpha = .01$ [.05/5].

Physiological treatment effects. To determine the physiological effects of the stress and control treatments, we analyzed the effects of participants' assignment to the stress or no-stress treatment on the time-course of log-transformed levels of sAA and salivary cortisol throughout testing in mixed ANOVAS involving the factors treatment (stress vs. no-stress) and cycle (1–10). ANOVA results are listed in Table 8. Due to incomplete salivary data, one subject from the stress treatment was excluded from the analysis of sAA and one subject in the no-stress treatment was excluded from the analysis of salivary cortisol and sAA.

We found no statistically significant differences in the time course of sAA levels between groups: Time × Treatment interaction, $F(4, 304) = 1.58$, $p = .179$, $\eta_p^2 = .02$. Visual inspection suggested no substantial differences in sAA levels between the stress and no-stress groups over the course of testing: −1 min: $d = 0.25$; +1 min: $d = 0.29$; +10 min: $d = 0.45$; +30 min: $d = 0.41$; +50 min: $d = 0.28$ (Figure 14 A).

Time courses of cortisol levels differed between groups: Time × Group interaction, $F(4, 308) = 21.06$, $p < .001$, $\eta_p^2 = .21$. Visual inspection suggested a small difference in cortisol levels between the stress and no-stress groups at time point −1 min: $d = 0.37$. At the remaining measurement time points, cortisol levels were substantially higher in the stress than in the no-stress group: +1 min: $d = 0.98$; +10 min: $d = 1.88$; +30 min: $d = 1.65$; +50 min: $d = 1.80$.

Psychological treatment effects. To determine the psychological effects of our treatments, we conducted mixed-design repeated-measures ANOVAs including the factors treatment (stress vs. no-stress) and time on participants' scores for the MDBF scales *good mood vs. bad mood*, *calmness vs. restlessness*, *and alertness vs. fatigue*. Due to incomplete questionnaire data, we excluded one subject in the no-stress group from the analyses of all MDBF scales, one subject in the stress group from the analyses of good mood and alertness, two additional participants in the stress group from the analysis of calmness and another subject in the stress group from the analysis of alertness.

We found a statistically significant treatment effect in the time course of good mood, $F(4, 304) = 25.48$, $p < .001$, $\eta_p^2 = .25$. Visual inspection revealed worse mood in the stress group at time points +1 min: $d = 1.78$, and +10 min: $d = 0.92$, and no difference at other measurement time points: −1 min: $d = 0.39$; +30 min: $d = 0.31$; +50 min: $d = 0.36$ (Figure 14 B). Similarly, acute stress affected the time course of subjective calmness, $F(4, 300) = 22.40$, $p < .001$, $\eta_p^2 = .23$, with lower calmness in the stress group at time points +1 min: $d = -1.70$, and +10 min: $d = -0.88$. Groups did not differ in calmness levels at other time points: −1 min: $d = -0.24$; +30 min: $d = -0.13$; +50 min: $d = -0.14$. Treatment did not affect the time course of alertness, $F(4, 300) = 0.72$, $p = .550$, $\eta_p^2 = .01$.

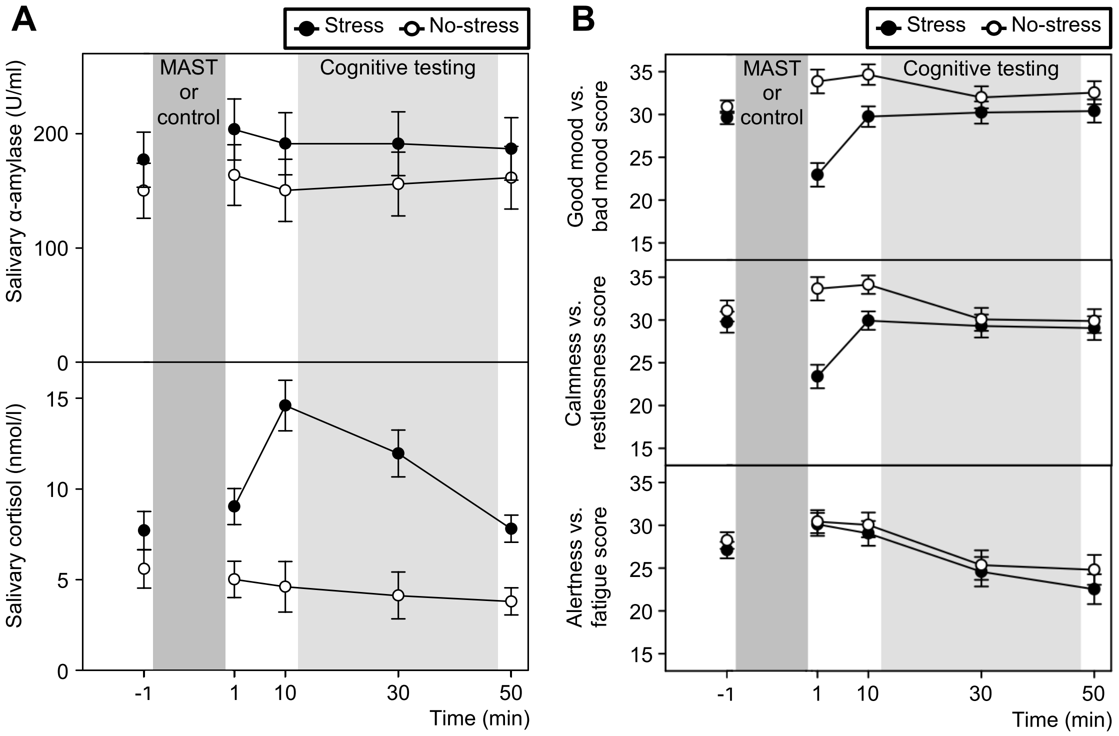

Figure 14. Stress response. (A) Mean salivary α-amylase, cortisol levels and (B) mean mental state scores on the three subscales *good mood vs. bad mood*, *calmness vs. restlessness*, *alertness vs. fatigue* from the German "Mehrdimensionaler Befindlichkeitsfragebogen" (MDBF, English version: Multidimensional mental-state questionnaire; Steyer et al., 1997) for the stress group and the no-stress group over the time-course of the experimental session (minutes before or after the Maastricht Acute Stress Test [MAST] or the control condition, respectively). Error bars represent standard errors of the between-group difference at each measurement time point.

Table 8

ANOVA results for the log-transformed physiological stress-response measures.

Stress-response measure	*dfs*	*MSE*	*F*-value	η_p^2	*p*-value
Salivary α-amylase (log-transformed)					
Treatment: no-stress vs. stress (0/1)	1, 76	3.86	2.60	.03	
Cycle (1−10)	4, 304	0.09	2.32	.03	
Treatment × Cycle	4, 304	0.09	1.58	.02	.179
Salivary cortisol (log-transformed)					
Treatment: no-stress vs. stress (0/1)	1, 77	1.39	37.50	.33	
Cycle (1−10)	4, 308	0.26	13.62	.15	
Treatment × Cycle	**4, 308**	**0.26**	**21.06**	**.21**	**< .001**

Note. In this table, *p*-values are reported only for a-priori defined hypothesis tests. Statistically significant treatment effects are printed in bold font ($p < .01$ [.05/5]).

Acute stress effects on PM. As described in more detail in the method section, we conducted our main analyses on log-transformed RTs for correct responses for PM-cue identification (STRG–key release in PM trial), output monitoring (subjective counting) and ongoing-task performance. Errors refer to the number of PM-response omissions, repetition or subjective-counting errors and errors in ongoing-task performance, respectively. To determine statistical significance of acute stress effects on these measures, we recycled the sum of *p*-value thresholds from significant treatment checks (α = .03 [.01*3]) and

Bonferroni-corrected this for multiple testing, which resulted in a p-value threshold of $\alpha = .005$ [.03/6] for each test of acute stress effects on PM. For each statistically significant acute stress effect, we conducted an additional analysis at the same p-value threshold to test whether this effect was modulated by ongoing-task demands.

PM-cue identification. Overall, participants correctly identified 71.8% of PM trials. In the remaining PM trials, participants gave either ongoing-task responses (27.8%) or no response (0.4%). Treatment did not affect the estimated mean RT for PM-cue detection, $e^B = 1.03$, 95% CI [0.97, 1.10], $p = .277$, $BF_{10} = 1.034$ (stress: 607 ms, 95% CI [568 ms, 647 ms], no-stress: 587 ms, 95% CI [551 ms, 623 ms]). There was no interaction between treatment and ongoing-task demand in RT data, $e^B = 0.95$, 95% CI [0.90, 1.01]. Similarly, treatment did not affect the estimated odds for PM-cue identification errors, $OR = 0.89$, 95% CI [0.63, 1.26], $p = .525$ (error probabilities, stress: 18.0%, 95% CI [13.6, 22.3], no-stress: 20.0%, 95% CI [15.6, 23.7]). There was no interaction between treatment and ongoing-task demand, $OR = 1.28$, 95% CI [0.90, 1.82].

Output monitoring. Participants gave an output-monitoring response in 98.9% of correctly detected PM trials (objective hits: 70.7%, subjective hits: 24.1%) and rarely made repetition errors (4.6%) or subjective counting errors (0.6%). Treatment did not affect RTs for output-monitoring responses, $e^B = 0.98$, 95% CI [0.91, 1.06], $p = .631$ (stress: 1046 ms, 95% CI [972 ms, 1119 ms], no-stress: 1064 ms, 95% CI [995 ms, 1134 ms]). Ongoing-task demands did not interact with treatment, $e^B = 0.97$, 95% CI [0.90, 1.04]. Mirroring RT data, treatment did not affect the odds of making repetition or subjective counting errors, $OR = 0.90$, 95% CI [0.39, 2.08], $p = .800$, and there was no Treatment × Ongoing-task demand interaction, $OR = 0.65$, 95% CI [0.23, 1.84].

Ongoing-task performance. Accuracy of ongoing-task performance was high: Participants gave a wrong response in 8.4% and omitted a response in 1.0% of ongoing-task trials. As revealed by a significant Treatment × Block interaction in the RT analysis, $e^B = 0.96$, 95% CI [0.95, 0.98], $p < .001$, $BF_{10} > 150$, PM-monitoring costs were reduced by 29% in the stress group (45 ms) compared to the no-stress group (64 ms). This effect was not qualified by ongoing-task demands, $e^B = 1.02$, 95% CI [1.00, 1.04], $p = .106$, $BF_{10} = 1.503$. Monitoring costs in error probabilities did not differ significantly between groups (stress: 2.0%, no-stress: 3.0%), Treatment × Block interaction, $OR = 0.94$, 95% CI [0.75, 1.17], $p = .560$. There was no Treatment × Block × Ongoing-task demands interaction, $OR = 1.03$, 95% CI [0.78, 1.35].

5.5.3. Exploratory Analysis

Commission errors. Additionally, we performed an exploratory analysis of acute stress effects on the risk of making a commission error after intention completion. Commission errors were rare: During test blocks, participants erroneously released the STRG key in 17 trials and in 5 trials also gave an output-monitoring response. Surprisingly, these commission errors occurred exclusively in ongoing-task trials. To determine acute stress effects on the risk of making a commission error, we compared the proportion of participants who made at least one commission error after treatment between the stress and the no-stress group (see also Möschl et al., 2017). we found that the commission-error risk did not differ between the stress group (5 participants) and the no-stress group (6 participants), $\chi^2(1) = 0.14$, $p = .710$, OR = 0.78, 95% CI [0.22, 2.82].

Time-dependent stress effects. Finally, we performed an exploratory analysis of time-dependent acute stress effects on PM. For this, we repeated our main analysis but included interactions of our main factors of interest with the factor cycle (1–10). Note that since we coded all participants as belonging to the control group in cycles 1–2, the exploratory models estimate pre-treatment performance of the stress group on the basis of post-treatment performance (cycles 3–10). Due to the low number of ongoing-task errors (9.4%), there were too few data points within each cell of the analysis model to obtain estimates and tests of the model parameters. Hence, we do not report an analysis of error rates in ongoing-task performance here. The point estimates and standard errors of the untransformed regression coefficients for these exploratory analyses are listed in Appendix C, Table C4. The analyses revealed small changes (1–2 %) over time in treatment effects on PM-cue identification and ongoing-task performance. That is, we found stable RTs for PM-cue identification over time after the stress treatment whereas PM responses in the control group became slightly faster over time, $e^B = 1.02$, 95% CI [1.00, 1.03], $p = .045$, $BF_{10} = 2.636$.

As suggested by a Cycle × Treatment × Block interaction on ongoing-task performance RTs, monitoring costs in the stress group remained stable over the course of testing, while monitoring costs in the control group became smaller over time, $e^B = 1.01$, 95% CI [1.00, 1.02], $p = .001$, $BF_{10} = 53.256$. A Cycle × Treatment × Block × Ongoing-task demand interaction suggested that this effect occurred mainly under low ongoing-task demands, as under high ongoing-task demands monitoring costs in both groups seemed stable over time, $e^B = 0.99$, 95% CI [0.98, 1.00], $p = .033$, $BF_{10} = 3.268$.

Table 9

Regression of the primary performance measures on various baseline covariates and treatment contrasts.

Performance measure	Correct response time (RT)				Error probability (ERR)			
	M(B)	*SE*(B)	*t*-value	*p*-value	*M*(B)	*SE*(B)	*z*-value	*p*-value
PM-cue identification								
Intercept	6.375	0.031	206.551		−1.407	0.132	−10.692	
Cycle (1 – 10)	−0.025	0.003	−8.441		0.035	0.017	2.151	
PM cues per cycle (3 – 5)	0.009	0.012	0.759		−0.241	0.069	−3.493	
N-back match trial (yes/no)	−0.110	0.016	−6.946		0.010	0.092	0.110	
Ongoing-task demand: low vs. high (0/1)	0.108	0.019	5.695		0.755	0.112	6.753	
Treatment: no-stress vs. stress (0/1)	0.034	0.031	1.087	.277	−0.112	0.176	−0.636	.525
Treatment × Ongoing-task demand	−0.050	0.031	−1.607		0.246	0.179	1.372	
Output monitoring								
Intercept	6.97	0.033	211.641		−3.995	0.363	−11.018	
Cycle (1 – 10)	−0.054	0.004	−15.070		−0.036	0.046	−0.777	
PM cues per cycle (3 – 5)	−0.022	0.014	−1.543		0.368	0.195	1.888	
N-back match trial (yes/no)	0.001	0.019	0.034		−0.135	0.248	−0.545	
Ongoing-task demand: low vs. high (0/1)	0.046	0.023	1.962		−0.175	0.283	−0.619	
Treatment: no-stress vs. stress (0/1)	−0.018	0.036	−0.481	.630	−0.109	0.430	−0.253	.800
Treatment × Ongoing-task demand	−0.034	0.037	−0.919		−0.428	0.530	−0.808	
Ongoing-task performance (PM monitoring)								
Intercept	6.155	0.023	265.015		−3.413	0.088	−38.691	
Cycle (1 – 10)	−0.022	0.001	−41.463		−0.034	0.006	−5.886	
PM cues per cycle (3 – 5)	−0.002	0.002	−0.820		−0.095	0.023	−4.136	
N-back match trial (yes/no)	0.016	0.003	5.540		1.552	0.029	52.778	
Ongoing-task demand: low vs. high (0/1)	0.183	0.006	32.935		0.930	0.071	13.139	
Block: test block vs. PM block (0/1)	0.133	0.005	27.491		0.699	0.066	10.563	
Treatment: no-stress vs. stress (0/1)	−0.035	0.008	−4.544		−0.286	0.108	−2.637	
Ongoing-task demand × Block	−0.041	0.007	−5.874		−0.352	0.083	−4.241	
Treatment × Ongoing-task demand	0.010	0.009	1.076		0.177	0.120	1.478	
Treatment × Block	−0.037	0.008	−4.802	< .001	−0.067	0.115	−0.583	.560
Treatment × Ongoing-task demand × Block	0.018	0.011	1.591	.112	0.029	0.140	0.208	

Note. In this table, *p*-values are reported only for a-priori defined hypothesis tests that were conducted in the main analysis. Statistically significant treatment effects are printed in bold font (*p* < .005 [.03/6]; see second-level analyses).

Table 10

Estimates for primary performance measures (standard errors in parentheses).

Performance measure	Stress				No-stress			
	RT(ms)		Errors (%)		RT(ms)		Errors (%)	
	1-back	2-back	1-back	2-back	1-back	2-back	1-back	2-back
PM-cue identification	607 (20)	644 (22)	18.0 (0.2)	37.3 (3.2)	587 (18)	654 (21)	19.7 (2.1)	34.3 (2.8)
Output monitoring	1046 (37)	1058 (39)	1.6 (0.6)	0.9 (0.4)	1064 (35)	1114 (38)	1.8 (0.6)	1.5 (0.6)
Ongoing-task performance (PM monitoring)								
PM block	504 (10)	586 (12)	4.4 (0.4)	9.3 (0.6)	537 (11)	609 (12)	6.2 (0.4)	10.6 (0.7)
Test block	459 (9)	548 (11)	2.4 (0.2)	7.0 (0.6)	473 (10)	560 (11)	3.2 (0.3)	7.7 (0.6)

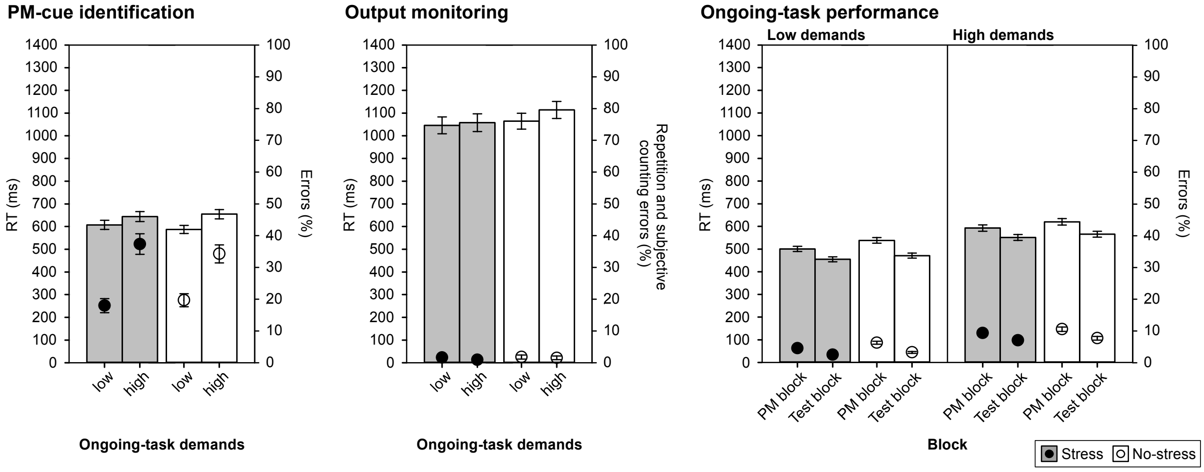

Figure 15. Cognitive-task performance. Estimated mean response times (RT, bar plots) and error probabilities (dot plots) for different performance measures as a function of ongoing-task demands (low vs. high) and treatment (stress vs. no stress). Data was estimated for averaged levels of *n*-back-target type with 4 PM cues per block at approximately 30 min after treatment. (A) PM-cue identification performance (i.e., STRG key releases in PM trials). Errors represent the probability of response omissions or ongoing-task responses in PM trials. (B) Output-monitoring performance for correctly identified PM cues (i.e., indicating the number of PM-cue encounters within a block). Errors represent the probability of repeating a PM-cue counting response from the preceding PM trial (repetition errors) or giving a lower PM-cue counting response than in the preceding PM trial. (C) Ongoing-task performance (PM monitoring) in ongoing-task trials during PM blocks and test blocks. Errors represent the probability of response-omissions, erroneous ongoing-task responses and erroneous PM responses. Error bars represent standard errors of the estimated marginal means.

5.6. Discussion

Our aim was to test the effects of acute stress on different components of PM under varying ongoing-task demands. For this, we assessed PM-cue identification, output monitoring and PM monitoring during low-demanding and high-demanding ongoing tasks in individuals that were exposed to a standardized stress-induction protocol (MAST) or a standardized control condition. We hypothesized that acute stress induction would either impair PM-cue identification, output monitoring and PM monitoring (Hypothesis 1) or enhance PM-cue identification and PM monitoring but at the same time impair output monitoring (Hypothesis 2). We assumed that under high ongoing-task demands, performance impairments should be exacerbated while performance benefits should be attenuated. Additionally, we explored stress effects on the risk of making a commission error after PM-task completion and a potential time-dependence of acute stress effects on PM.

Results were straightforward and the effects of acute stress on PM were largely consistent with previous findings: Stress did not affect PM-cue identification. In addition, we showed for the first time that acute stress had no effect on output-monitoring responses in an event-based PM task. Most importantly, we replicated our earlier result of smaller PM-monitoring costs in stressed than in non-stressed individuals (Möschl et al., 2017). Moreover, this effect was not modulated by ongoing-task demands. Our exploratory analyses revealed no hints for an effect of acute stress on the risk of making a commission error after intention completion with the nonsalient and nonfocal PM cues in the present experiment, and small effects that suggested that in stressed individuals RTs for PM-cue identification and PM-monitoring costs remained stable, while these measures decreased over time in non-stressed individuals.

While this pattern of results was not predicted by either of our hypotheses, it speaks against a general stress-induced impairment of higher-order cognitive functioning. Instead, it provides support for the notion of selective stress effects on distinct sub-processes of PM functioning. We will discuss our main findings and their implications in more detail below.

As indicated by our treatment checks these findings did likely not result from a lack of acute stress responses after MAST exposure. That is, increased levels of salivary cortisol as well as short-lived decreases in subjective good mood and increases in restlessness after the stress compared to no-stress treatment indicated successful stress induction. Acute stress did not affect subjective alertness, suggesting that stress effects on PM were not due to differences in this variable (e.g., Plessow et al., 2012; Weckesser et al., 2016). In contrast to reports of short-lived increases in sAA levels after stress induction (Engert et al., 2011; Plessow et al., 2012; Walser et al., 2013), we did not observe an effect of the stress treatment on sAA levels. This was most likely due to high variance in baseline sAA levels and sAA reactivity (Schoofs et al., 2008).

Additionally, our analyses of the cognitive task revealed medium to high PM-cue identification rates, evidence for PM-monitoring costs and a low number of output monitoring errors. High compared to low ongoing-task demands produced substantial impairments in PM-cue identification, which dovetails with previous findings (e.g., Meier & Zimmermann, 2015). By contrast, high ongoing-task demands produced only a slight slowing of correct output-monitoring responses. Lastly, high ongoing-task demands decreased PM-monitoring costs in both RTs and error rates. Although we do not have a strong explanation for this, one could speculate that this effect was due to substantially increased RTs and error rates in PM and test blocks under high ongoing-task demands.

5.6.1. Acute Stress Effects on PM-Cue Identification

While our finding of preserved PM-cue identification under acute stress mirrors results from previous studies with event-based PM tasks (Möschl et al., 2017; Nater et al., 2006; Walser et al., 2013), they conflict with recent reports of altered PM-cue identification under acute stress (Glienke & Piefke, 2016; Szőllősi et al., 2018).

First, our finding stands in contrast to a study by Glienke and Piefke (2016) who found that stressed compared to non-stressed individuals exhibited higher PM accuracy in a real-life simulation. It is important to note that they assessed PM performance starting at about 60 min after stress induction, while we assessed PM performance 15–55 min after stress induction. This difference in methodology might be critical since acute stress effects on PM may depend on the time-frames of investigation, as has been observed, for instance, in research on working memory and cognitive flexibility (e.g., Hermans et al., 2014; Plessow, Fischer, et al., 2011).

Additionally, participants in the Glienke and Piefke (2016) study had to maintain several PM tasks over a long period of time (50 min) and did not perform an ongoing task, but had to coordinate performance of event- and time-based tasks while reminders for both were present. While the duration of PM-task maintenance is known to affect PM performance (e.g., McBride, Beckner, & Abney, 2011), future research might determine in what ways other variables like the need to coordinate multiple PM tasks at once, performing an ongoing task or being in the constant presence of PM cues might affect the manifestation of acute stress effects on PM.

Second, our finding stands in contrast to a study by Szőllősi et al. (2018) who observed faster PM responses in a nonfocal event-based PM task after stress induction with the Socially Evaluated Cold Pressor Test (SECPT; e.g., Schwabe & Schächinger, 2018). This discrepancy was quite surprising, considering that we also used a nonfocal PM task and a stress induction protocol that, similar to the SECPT, combines both physiological and psychosocial stressors (Smeets et al., 2012). Szőllősi et al. (2018) argued that the use of different stress-induction protocols might account for the differences in acute stress effects on PM performance across studies. Indeed, to our knowledge, acute stress effects on event-based PM performance were observed only in two studies that used the SECPT (Glienke & Piefke, 2016; Szőllősi et al., 2018). By contrast studies that used the Trier Social Stress Test (Kirschbaum et al., 1993) did not observe any acute stress effects on event-based PM performance (Möschl et al., 2017; Nater et al., 2006; Walser et al., 2013). Although these stress induction protocols produce similar residual stress effects—i.e., worse mood and increased activity of the sympathetic nervous system—the SECPT typically elicits a smaller cortisol response than the Trier Social Stress Test or the MAST (Schwabe & Schächinger, 2018; Smeets et al., 2012). This difference might be crucial, as cortisol and residual stress effects not only have different effects on cognitive functions (Shields et al., 2016), but cortisol is thought to counteract residual effects of acute stress (Hermans et al., 2014; Weckesser et al., 2016). So far, however, there is only preliminary support for this idea: A reanalysis of data by Glienke and Piefke (2017), revealed that participants with low cortisol responses showed stable and ultimately more accurate PM performance over time than participants in the control group. By contrast, participants with high cortisol responses showed fluctuating PM performance over time compared to the control group. Under the assumption that decreased cortisol responses translate into a heightened influence of residual stress effects, their finding suggests that acute stress effects on PM performance might primarily result from residual stress effects on cognition. Unfortunately, the present study does not allow drawing a clear conclusion regarding the impact of different stress responses on PM. On the one hand, the lack of acute stress effects on PM-cue identification in the present study might be due to the lack of a systematic increase of sympathetic nervous system activity as measured by sAA levels after stress induction. On the other hand, it might be the result of different intensities of cortisol responses and residual stress effects following from different methods of stress induction in other studies.

It should be pointed out that at the moment there are simply too few studies on this topic to evaluate effects of different stress responses and stress-induction protocols on PM. To address this question, future research would need to systematically assess the effects of different stress responses on PM. This could be achieved, for instance, by assessing in what ways different stress-induction protocols or medically induced increases of sympathetic nervous system and hypothalamus-pituitary-adrenal axis activity affect performance in comparable PM tasks (see Weckesser et al., 2016, for such an approach). This would also allow to more clearly isolate the source or mechanisms of stress effects on PM. For instance, a mediation of acute stress effects on PM by residual stress effects, would likely reflect stress-induced alterations of cognitive-control functions, whereas a mediation by cortisol would likely reflect stress-induced improvements of visual attention (e.g., Putman, Hermans, & van Honk, 2010).

5.6.2. Acute Stress Effects on Output Monitoring

Given the potential link between output-monitoring and stress-sensitive episodic memory (Ball et al., 2018), we assumed that acute stress would impair output monitoring in PM. In contrast to our hypotheses, we did not find any effects of acute stress on output monitoring in PM. This was especially surprising given that we induced stress prior to memory encoding, which in research on episodic memory has been shown to produce impairments (for an overview, see Shields et al., 2017). When interpreting this finding, it is important to keep in mind that, compared to previous research, hit rates in output-monitoring responses were exceptionally high (94.8%). Hence, based on the present study, we cannot determine whether the lack of a stress effect on output monitoring was simply due to a ceiling effect or whether it represents a genuine invulnerability of output monitoring towards effects of acute stress. Note, that this is a theoretically important question related to the underlying processes of output monitoring in PM. Specifically, if acute stress had no effect on output monitoring, but did affect episodic memory, then this dissociation would suggest that output monitoring most likely does not or only to a small extent rely on episodic memory. To test this, future studies could investigate stress effects on both output monitoring and episodic memory under certain conditions or in certain groups (e.g., older adults) that produce considerable variability in output-monitoring responses or episodic memory abilities, respectively (Koriat et al., 1988; Marsh et al., 2007; Skladzien, 2010).

5.6.3. Acute Stress Effects on Ongoing-task Performance (PM Monitoring)

To our knowledge, the present study is the second demonstration of a stress-induced reduction of PM-monitoring costs in a nonfocal event-based PM task (Möschl et al., 2017), while one study did not find such an effect in a nonfocal event-based PM task (Szőllősi et al., 2018). On speculative terms, this discrepancy could be attributed to a more pronounced cortisol secretion in the TSST or MAST than in the SECPT (Schwabe & Schächinger, 2018) that was used by Szőllősi et al. (2018). Importantly, however, as with acute stress effects on PM-cue identification, future research would be necessary to determine the individual contributions of sympathetic nervous system and hypothalamus-pituitary-adrenal axis activity to acute-stress effects on PM-monitoring costs. Note also that our study does not allow to clearly determine isolated effect of physiological or psychosocial stress effects on ongoing-task performance, given that the MAST included both types of stressors. However, in light of similar findings after TSST treatments that only includes psychosocial stressors (Möschl et al., 2017), it at least seems that this effect does not primarily arise from residual stress effects or physiological stress induction.

Alternatively, the discrepancy between studies might result from differences in monitoring demands. That is, previous studies only observed acute stress effects on PM-monitoring costs in demanding nonfocal but not in focal PM tasks (Möschl et al., 2017; Nater et al., 2006; Walser et al., 2013), which suggests that this stress effect on PM monitoring depends on the difficulty to monitor for PM cues. Hence, one might ask whether monitoring demands in the study by Szőllősi et al. (2018) were lower than in the present study even though they also used nonfocal PM tasks. Relatedly, it is feasible that Szőllősi et al. (2018) did not observe a statistically significant stress effect on PM-monitoring costs, since power was too low to detect such a small effect (η_p^2 = .01, p. 87) after losing all trial-specific information due to data aggregation in ANOVA. By contrast, the present study likely provided sufficient power to also detect small stress effects on PM-monitoring costs, given that our analyses were based on substantially more trial-specific information. Similarly, Möschl et al. (2017) likely observed a statistically significant stress effect on PM monitoring besides losing trial-specific information due to data aggregation in ANOVA since the effect was quite large (η_p^2 = .12, p. 59).

In the present study, several mechanisms for the stress-induced reduction of PM-monitoring costs are feasible. In a previous report of a similar finding, Möschl et al. (2017) argued that (a) acute stress might increase the efficiency of scanning stimuli for PM-cue defining features due to improved selective attention or an up-regulation of the salience network (Hermans et al., 2014) or (b) acute stress might shift processing strategies in PM towards relying more on automatic spontaneous intention retrieval (Schwabe & Wolf, 2013; Szőllősi et al., 2018), which would have reduced the mobilization of top-down controlled monitoring for PM cues. Alternatively, it is also possible that acute stress reduced the response threshold for ongoing-task performance (Heathcote et al., 2015; Strickland et al., 2017). That is, under acute stress participants might allot less time towards scanning stimuli for the presence of PM-cue defining features, which would also reduce PM-monitoring costs.

It is important to note that each of these mechanisms would have predicted corresponding changes in PM performance under acute stress: Improved selective attention, increased efficiency of cue scanning or a shift towards spontaneous retrieval should have produced faster or more accurate PM-cue identification; a decreased PM-response threshold should have also produced more false alarm responses. Based on the present study it is not possible to determine which of these processes accounts for the reduction in PM-monitoring costs under stress. To our knowledge, at the moment no study tested these processes in PM directly and there is only preliminary support for a stress-induced shift towards spontaneous retrieval in PM (Szőllősi et al., 2018). One way to tackle this important question in future studies could be to assess parameter changes in cognitive models of ongoing-task performance (e.g., Horn & Bayen, 2015; Strickland et al., 2017) after acute stress induction. This could at least provide information about in what ways acute stress changes response thresholds in ongoing-task performance and/or the resource allocation or attention distribution between PM and ongoing task. Alternatively, to test this in a more direct way, one could assess neurophysiological effects of acute stress during PM performance. Here, for instance, a stress-related increase of activation in hippocampal areas during ongoing-task performance with a PM task would likely reflect an increased reliance on spontaneous-retrieval based processing under stress (Cona et al., 2016; McDaniel et al., 2015).

5.6.4. The Role of Ongoing-Task Demands

Under the assumption that PM and ongoing-task performance rely on shared resources (Einstein & McDaniel, 2005; Scullin et al., 2013; R. E. Smith, 2003), we expected stronger effects of acute stress on PM when resources for PM performance are limited by a demanding ongoing task. In contrast to this hypothesis, even under high ongoing-task demands acute stress did not affect PM-cue identification or output monitoring. Similarly, stress effects on PM-monitoring costs in RTs were not modulated by ongoing-task demands.

This might indicate that PM and ongoing-task performance do not rely on shared resources. Specifically, recently the delay theory of PM proposed that adding a PM task to an ongoing task merely increases response thresholds for ongoing-task performance but does not usurp shared resources from it (Heathcote et al., 2015; Strickland et al., 2017). While recent modeling studies support this assumption (Strickland et al., 2017; cf. Anderson et al., 2018), at least in the present study processing resources for PM and ongoing-task performance seemed to have overlapped, given that high ongoing-task demands impaired PM performance.

Alternatively, one might speculate that the demanding ongoing-task in the present study might also have induced acute stress. This could in turn have overshadowed any effects of MAST exposure on PM. While manipulations of task difficulty or time pressure to perform a task have previously been employed to induce subjective stress (Chajut & Algom, 2003), it is unclear to which extent demanding cognitive tasks produce physiological stress responses. Hence, we cannot rule out this explanation.

As a third alternative, acute stress effects on PM might not be mediated primarily by changes in prefrontal-cortex functioning but rather by changes to occipital-cortex functioning. Specifically, by a stress-induced increase in the maintenance and perceptual amplification of stimulus information that is relevant for a (PM) task at hand (Weckesser et al., 2016; Weckesser, Enge, Riedel, Kirschbaum, & Miller, 2017). Consequently, ongoing-task demands or demands on prospective remembering (i.e., prospective load; Möschl et al., 2017) that are presumably associated with prefrontal cortex activation, would not affect the expression of acute stress effects on PM. This idea is somewhat corroborated by the fact that acute stress effects on PM have been mostly observed in nonfocal PM tasks that heavily rely on visual checking of stimuli for PM-cue features, but not in focal PM tasks in which PM-cue identification can occur rather spontaneously with a low need for such visual checking processes (Cona et al., 2016; McDaniel et al., 2015). So far, a few studies investigated this alternative route of acute stress effects on cognitive performance in partial report and dual tasks (e.g., Miller, Weckesser, Smolka, Kirschbaum, & Plessow, 2015; Weckesser et al., 2016) or visual discrimination tasks (e.g., Shackman et al., 2011). Importantly, however, to our knowledge there is no study of acute stress effects on visual processing in the PM literature. Future studies could test whether acute stress effects on PM are mediated by changes in visual-sensory processing, for instance, by investigating acute stress effects on PM performance under different degrees of stimulus degradation.

5.6.5. Explorative Analysis of Commission Errors

Previous research suggested that commission errors would be particularly rare when $\text{PM}_{\text{REPEATED}}$ cues are nonsalient and nonfocal (Scullin et al., 2012)—like in the present study. However, in contrast to previous studies (Möschl et al., 2017; Walser et al., 2013) and in order to not foster intention deactivation we did not provide error feedback for commission errors in the present study. Despite this, commission errors were rare and acute stress did not affect participants' commission error risk. It is most likely that acute stress only has an effect on intention deactivation and commission errors when task features foster a rather automatic or spontaneous reactivation of completed intentions—for instance, with salient PM cues (McDaniel et al., 2015). Therefore, when trying to assess acute stress effects on both commission errors and PM functioning, one should be aware of the trade-off between testing one or the other (Walser et al., 2013). Specifically, conditions under which acute stress might affect commission errors—i.e., salient and focal PM cues—seem to be unsuitable to assess acute stress effects on PM (Möschl et al., 2017).

5.6.6. Explorative Analysis of Time-dependent Stress Effects

Previous studies reported that acute stress effects on cognitive functions change with increasing time distance to a stressor (e.g., Plessow, Fischer, et al., 2011). Here we found that stressed individuals showed slightly more stable PM response speed and PM-monitoring costs in ongoing-task RTs over time than non-stressed individuals. By contrast, the absence of stress effects on other performance measures did not change over time (see also Walser et al., 2013). While these findings dovetail with previous reports of stable PM performance over time after stress induction (Glienke & Piefke, 2016), in light of their almost negligible size, they should be interpreted with caution. Unfortunately, we could not estimate time effects on PM-monitoring costs in ongoing-task error rates. However, it seems likely that we would not have found a time effect here, given that several previous studies reported an absence of PM monitoring costs in ongoing-task error rates (e.g., Loft, Kearney, & Remington, 2008; Moyes, Sari-Sarraf, & Gilbert, 2019; Rummel et al., 2017).

5.6.7. Conclusion

The present study was the first to investigate the effects of acute stress on output monitoring in PM and the role of ongoing-task demands for PM functioning under stress. In line with recent meta analyses (Shields et al., 2015, 2016), our findings support the notion, that although stress impairs most cognitive-control functions, it does not universally impair all of them. Additionally, our findings corroborate ideas that acute stress only acts on specific sub-processes of higher-order cognitive functions and does so mostly under high demands on these sub-processes.

Our findings are partially in line with theories of acute stress effects on cognition. Specifically, reduced PM-monitoring costs under stress (Möschl et al., 2017) and a stress-related increase in PM-response speed (Szőllősi et al., 2018) can be explained by a stress-induced shift towards automatic and habitual processing (Schwabe & Wolf, 2013; Vogel, Fernández, Joëls, & Schwabe, 2016). Albeit, these effects could also be explained by stress-induced changes in visual-sensory processing (Weckesser et al., 2016, 2017). Most importantly, however, so far no theoretical account of acute stress effects on cognition is able to explain the complex pattern of effects and non-effects in different sub-components of PM without expansion. That is, while each model predicts changes in more than one sub-component of PM, in extant research on this topic, stress only affected one sub-component in each task—either PM response speed, PM accuracy or PM monitoring—while the remaining components remained unaffected (Glienke & Piefke, 2016; Möschl et al., 2017; Szőllősi et al., 2018).

In summary, the previous two chapters showed that, while acute stress may have no effect on our abilities to retrieve postponed intentions and to remember past actions, it might make us more susceptible to making commission errors when intention deactivation becomes demanding. More importantly, acute stress seems to increase the efficiency with which we monitor our environment for retrieval opportunities. Future studies are needed to determine (a) the mechanisms behind this improvement in monitoring efficiency, (b) in what way the type and extent of physiological stress responses affect the direction and extent of acute stress effects on PM and (c) to identify the route of action through which these effects arise.

6. General Discussion

The overarching aim of my dissertation was to advance our understanding and knowledge about mechanisms and modulators of both prospective memory and intention deactivation. Specifically, I aimed to identify potential mechanisms underlying the deactivation of completed intentions, test the relevance of cognitive control for this and assess the reliability of PM and intention deactivation under acute stress. To these aims, I conducted a comprehensive review of the studies on intention deactivation that had been published since its arrival in the PM literature in 1998 (Marsh et al., 1998) and, based on these studies, evaluated the validity of the documented accounts for aftereffects of completed intentions and mechanisms underlying intention deactivation. Subsequently, in Study 1, I assessed the relevance of cognitive-control availability for intention deactivation by manipulating transient cognitive-control demands during encounters of $\mathrm{PM}_{\mathrm{REPEATED}}$ cues in both younger and older adults. Lastly, to further elucidate the reliability of PM and intention deactivation under conditions that are known to affect cognitive-control functioning, in Study 2 and Study 3 I assessed the effects of acute stress experiences in different stress-induction protocols on several components of PM as well as on aftereffects of completed intentions in differently demanding situations. In the following chapters, I will first summarize and discuss the outcomes of these individual projects, then discuss implications of my findings for PM research as well as their limitations and lastly will provide a comprehensive outlook of where this research could or should be headed next.

6.1. Mechanisms of Intention Deactivation

The central aims of my review in Chapter 2 were to provide an up-to-date overview of research on intention deactivation and identify mechanisms and modulators of intention deactivation and aftereffects of completed intentions. Regarding the underlying mechanisms of aftereffects, I found that aftereffects are likely not merely the result of poorly understood or miscommunicated task instructions or a lack of time or resources for postactional processing and they cannot be explained by continued monitoring for PM cues alone. Although these factors play a role in determining the extent of intention-deactivation failures, it seems to be minor. The overall picture of the reviewed studies suggests that, at least in event-based PM tasks, aftereffects of completed intentions can at the moment best be explained by continued retrieval of completed intentions that is triggered by PM$_{\text{REPEATED}}$ cues. I based this conclusion on the observations that (a) in event-based PM tasks, aftereffects have repeatedly been observed in terms of commission errors that directly indicate full intention retrieval; and that (b) previous studies consistently found that the same factors that modulate retrieval of active intentions also modulate aftereffects of completed intentions. In fact, aftereffects seem to be most likely under conditions that foster spontaneous intention retrieval during active PM phases. Here, key factors for the occurrence of aftereffects are a strong association between PM cues and intended actions, a high salience of PM cues, a high overlap between operations required to detect a PM cue and operations required to perform the ongoing task (i.e., with focal PM cues) and a strong context match between the tasks during which an intention is actively pursued and the tasks during which its deactivation is measured. Variations in these factors can also account for a large portion of the divergence between findings in aftereffects research with event-based PM tasks.

Regarding the mechanisms underlying aftereffects in script-based paradigms or event-based paradigms that assess memory accessibility of semantic associates of PM cues, the reviewed studies showed that the accessibility of intention representations often goes to or below a neutral baseline after intention completion—that is, a deactivation or inhibition of completed intentions. Here, I discussed a selection of possible explanations for the seeming discrepancy between these findings and findings of continued intention retrieval in event-based PM tasks. At the moment, the most viable explanation for this is that intentions are in principle deactivated or inhibited after completion, but that this process can sometimes be *incomplete*. Consequently, findings of continued retrieval of completed intentions in event-based PM tasks occur because this general deactivation or inhibition process did not (yet?) target PM-cue–action associations and/or it only fully worked in some, but not all participants.

To summarize, across the 34 studies I reviewed in Chapter 2, I found that although aftereffects of completed intentions can occur in terms of decreased memory accessibility of intention-related semantic concepts, impaired ongoing-task performance in PM$_{\text{REPEATED}}$ trials or commission errors, they seem to occur at a rate or to an extent that does not completely shut down subsequent task performance and pursuit of novel goals. This mirrors experiences of flexibly switching to different intentions or goals in everyday life and suggests that completed intentions are in fact deactivated (or inhibited) at some point in time. This process, however, can be faulty, incomplete or take a long time. That is, instead of assuming a dichotomy of either inhibition or continued intention retrieval, I argue that intention deactivation moves along a continuum between a full *re*-activation and a full *de*-activation or inhibition. Critically, this continuum is shifted by a multitude of factors that affect retrieval of intentions and perhaps also cognitive control over it. Consequently, the central question guiding research on intention deactivation should not be *whether* completed intentions are inhibited or continue to be retrieved, but *when* and *under what circumstances*?

6.2. The Relevance of Cognitive Control for Intention Deactivation

As I showed in Chapter 2, one open question regarding the mechanisms of intention deactivation is the involvement of cognitive-control processes. Building on correlative findings of increased commission errors in older adults with deficits in cognitive-control capabilities, a dual-mechanisms account of commission errors proposed that commission errors stem from failures of cognitive control over intention execution (Bugg & Scullin, 2013). Importantly, an association of intention deactivation with cognitive-control functioning should not only be visible in long-lasting cognitive-control deficits in older adults (Bugg et al., 2016) or during periods of fatigue (Scullin & Bugg, 2013), but also during transient demands on cognitive control during encounters of $\text{PM}_{\text{REPEATED}}$ cues. To test this assumption, in two experiments (Study 1), I parametrically manipulated cognitive-control demands in terms of transient demands on information processing and uncertainty reduction during a majority-function task. I reasoned that these cognitive-control demands would usurp or bind resources to "control" intention retrieval and/or intention execution particularly in $\text{PM}_{\text{REPEATED}}$ trials. Additionally, to further elucidate under which conditions older adults may show deficits in intention deactivation, in Experiment 1, I assessed the effects of this manipulation in interaction with effects of age in a group of younger and older adults.

Similar to previous studies, in both experiments, trial-by-trial cognitive-control demands affected ongoing-task performance (e.g., Fan et al., 2008) and nominally more older adults made a commission error than younger adults in Experiment 1 (e.g., Scullin et al., 2011). Most importantly, however, the manipulation of cognitive-control demands did not reliably affect aftereffects of completed intentions: In Experiment 1, aftereffects were smaller under medium cognitive-control demands than under low and high demands, suggesting a u-shaped modulation of intention deactivation. By contrast, in an extension of this manipulation in Experiment 2 aftereffects did not differ between demand levels.

Under the assumption that any demand on cognitive-control capabilities would interfere with or alter cognitive control over retrieval or execution of completed intentions, the findings from Study 1 suggest that the transient availability of cognitive control in $\text{PM}_{\text{REPEATED}}$ trials plays a much smaller role for intention deactivation than suggested by previous research (Bugg et al., 2016). Yet, concluding that intention-deactivation does not involve cognitive-control processes during $\text{PM}_{\text{REPEATED}}$ trials would be premature, given that Study 1 assessed only a single type of cognitive-control demand. Maybe preventing commission errors hinges specifically on response inhibition rather than on more general capabilities for information processing and uncertainty reduction tested here. Most recently, Schaper and Grundgeiger (2019) argued that this might not necessarily be the case, given that introducing time for response inhibition in between presentation of a $\text{PM}_{\text{REPEATED}}$ cue and the opportunity to give an ongoing-task response (i.e., 1–2 s response lags), did not reduce commission errors after PM-task performance had been cancelled. However, while they observed no commission errors when introducing a 1 s response lag after a PM tasks had been finished successfully, their study lacked a control group to assess commission errors after finished PM tasks without a response lag. Hence, future studies would be required to determine the involvement of response inhibition in the deactivation of completed intentions.

Alternatively, it could be that demands on information processing and uncertainty reduction affect inhibition of intention execution but, contrary to my expectations, not retrieval of the completed intention. If this was the case, the cognitive-control demands I manipulated in Study 1 should have increased commission error rates but not aftereffects in ongoing-task RTs. While the absence of control-demand effects on commission errors in Experiment 1 and a slight reduction of commission errors with increasing control demands in Experiment 2, speak against this, I cannot rule out this option based on these experiments, simply due to the very low number of commission errors in this study. Here future research would need to test the effects of this control demand under conditions that make commission errors more likely.

6.3. Acute Stress Effects on PM and Intention Deactivation

Surprisingly, despite the importance of PM for everyday life and the susceptibility of higher-order cognitive functioning to acute stress effects (e.g., Shields et al., 2016), at the time I started working on my dissertation, there were only three published studies that assessed acute stress effects on a small selection of PM sub-processes under presumably moderate demands (Nater et al., 2006; Piefke & Glienke, 2017; Walser et al., 2013), and only one study had also assessed intention deactivation (Walser et al., 2013). In order to extend our knowledge on this topic and in order to overcome some of the limitations of this research, in Study 2 I first tested the effects of acute psychosocial stress in the Trier social stress test (TSST; Kirschbaum et al., 1993) on PM-cue detection and intention retrieval as well as on PM monitoring and intention deactivation under varying task demands. In Study 3, I extended this approach and tested the effects of acute physiological and psychosocial stress in the Maastricht acute stress test (MAST; Smeets et al., 2012) on PM and output monitoring in PM under varying demands on concurrently performed ongoing tasks that themselves have been shown to affect PM functioning (e.g., R. E. Smith, Horn, & Bayen, 2012).

In line with previous research, Studies 2 and 3 showed that acute stress did not affect PM-cue detection and intention retrieval in event-based PM tasks (Nater et al., 2006; Walser et al., 2013). Extending this research, my studies showed that this was independent of demands on PM-cue detection and PM monitoring (Study 2) and independent of ongoing-task demands (Study 3). By contrast, depending upon demands on intention deactivation, acute stress seemed to exacerbate aftereffects of completed intentions. That is, in Study 2, acute stress nominally increased the likelihood of erroneous intention retrieval in terms of commission errors, when $\text{PM}_{\text{REPEATED}}$ cues were salient and focal and thus, difficult to ignore. Most importantly, however, both Study 2 and Study 3 showed that when PM-cue detection was difficult (nonsalient nonfocal PM cues), acute stress actually reduced costs in ongoing-task performance that are often attributed to the engagement of PM-monitoring processes (e.g., McDaniel et al., 2015; cf. Strickland et al., 2017). Study 2 suggested that acute stress effects on these PM monitoring costs occur only in nonfocal PM tasks in which PM-cue detection is visually demanding and in which PM performance is presumably associated with strong engagement of top-down control processes (e.g., Cona et al., 2015). The fact that I found a similar stress-related effect with substantially smaller PM-monitoring costs in Study 3 also suggests that the occurrence of stress effects on PM monitoring in nonfocal PM tasks is not a function of the size of costs. As I will discuss in more detail in Chapter 6.6.2, the specifics of the stress-induction protocols used in these studies might be an important factor for the occurrence of this effect, as recently Szőllősi et al. (2018) did not observe reduced PM-monitoring costs under stress despite also using a nonfocal PM task.

Taken together, Studies 2 and 3 made a substantial contribution to our understanding of the reliability of PM and intention deactivation under acute stress. They extend previous research by testing stress effects on multiple components of PM and assessing the relevance of PM-cue detection demands (i.e., prospective load, PM-cue salience, PM-cue focality), ongoing-tasks demands and demands on intention deactivation for the occurrence of acute stress effects on PM. The profile of stress effects on PM that I observed here—preserved intention retrieval but altered PM monitoring—fits the overall picture of stress effects on cognition conveyed by the literature to date: Acute stress effects selectively target specific cognitive functions and processes (e.g., Shields et al., 2016) and for some of them does so mainly under high task demands (e.g., Schoofs et al., 2008, 2009). For PM, my findings additionally suggest that acute stress might not necessarily target memory-dependent processes in PM, but might alter retrieval strategies or attentional processes. This dovetails with some of our everyday experiences that, rather than making us forget what we intended to do, stress changes the way we deal with PM demands: We may proactively set up easily identifiable external reminders when remembering postponed intentions under stress would otherwise be hard.

6.4. Implications for Views of PM Functioning

The research presented in this thesis provided an integrative and unifying perspective for research on aftereffects of completed intentions and substantially extended our knowledge about mechanisms and boundary conditions of successfully remembering and deactivating intentions. Beyond that, while several of my findings support popular views of PM functioning, they also give reason to question or fine-tune some of the assumptions they make. Therefore, in the following section, I will outline central implications of my research for currently dominant views of PM functioning.

6.4.1. Retrieval Processes in PM

One central claim of the multiprocess view of PM is that prospective remembering in event-based PM tasks can be supported by both a resource-saving, almost automatic process of spontaneous intention retrieval as well as a resource-demanding, top-down controlled process of monitoring for retrieval opportunities (e.g., McDaniel et al., 2015). This PM monitoring process in particular is thought to produce measurable costs in ongoing-task performance, due to capacity sharing with the ongoing task (e.g., Scullin et al., 2013; Shelton & Scullin, 2017; cf. Heathcote et al., 2015). Additionally, the relative engagement of both processes is thought to be modulated by the processing demands of a PM task—specifically, by the difficulty to maintain and detect PM cues. Consequently, variations in the engagement of PM monitoring and spontaneous retrieval coincide with variations in the accuracy of PM-cue detection (McDaniel et al., 2015). Across the three experiments reported here, I consistently found evidence for both (spontaneous) retrieval of intended actions as well as costs in ongoing-task performance when intentions were actively pursued. Consistent with the multiprocess view and previous findings, in Study 2 ongoing-task costs were higher in nonfocal PM tasks that placed additional processing demands on PM-cue detection than in focal PM tasks in which processing and detection of a PM cue can take place rather automatically (Einstein & McDaniel, 2005). This again coincided with descriptively higher accuracy of PM-cue detection in the focal PM tasks in Study 2 (approx. 90%) compared to the nonfocal PM tasks in Studies 2 and 3 (approx. 72–80%). Both findings support the notion that intention retrieval comprises a continuum from almost automatic spontaneous intention retrieval to mostly top-down controlled PM monitoring, depending on the features of a PM task.

Additionally, Study 2 provides further support for the notion of a flexible engagement of PM monitoring according to demands on PM-cue detection: PM monitoring costs in ongoing-task performance were larger under a high prospective load that required detecting PM-cues according to a complex rule (e.g., find words that contain any permutation of the letters T, R, & A) than under a low prospective load that required detecting PM cues according to a simple rule (e.g., find words that contain the syllable TRA).

Similar to the multiprocess view of PM, retrieval accounts of aftereffects of completed intentions that I outlined in Chapter 2 posit that the occurrence of aftereffects is more likely under conditions that foster spontaneous intention retrieval than under conditions that foster PM monitoring (e.g., Scullin et al., 2012). Corroborating these accounts, RT aftereffects and commission-error rates were largest with salient focal $\text{PM}_{\text{REPEATED}}$ cues in Study 2 than with non-salient nonfocal $\text{PM}_{\text{REPEATED}}$ cues in Studies 2 and 3.

6.4.2. Sources of Ongoing-Task Costs in PM Tasks

One central assumption of the multiprocess view of PM is that the occurrence and magnitude of ongoing-task costs in event-based PM paradigms critically depends on features of a PM task. Specifically, such costs are thought to mostly occur under conditions in which PM tasks pose high demands on PM-cue processing or cognitive control, for instance, when PM cues are nonsalient and nonfocal (McDaniel et al., 2015). Interestingly, however, I observed ongoing-task costs also in PM tasks in which PM cues were salient and focal (Study 2) and PM monitoring is typically considered to be minimal or absent. Meier and Rey-Mermet (2012, 2018) showed that ongoing-task costs in focal PM tasks can also arise from aftereffects of responding to PM cues—particularly in the first trial after a PM cue. In Study 2, however, conflation of ongoing-task costs with this type of aftereffects should be minimal, since the analyses excluded two trials after each PM cue (Brewer, 2011). Alternatively, in line with the multiprocess view, the presence of ongoing-task costs in the focal PM task in Study 2 could again be attributed to the features of the experimental setup. I assessed PM performance across multiple PM trials and during multiple cycles; I did not raise importance of the ongoing-task performance; and participants in this study were likely aware that they would be performing multiple PM tasks over the course of the experiment. Additionally, although PM cues in the focal condition of Study 2 were presented in the same salient color, which should have minimized the need for PM monitoring, participants had a heightened prospective load that required them to detect four PM cues instead of one. Compared to single PM tasks with a single PM cue and emphasis on ongoing-task performance, according to the multiprocess view, the features of my experiment in Study 2 presumably shifted the balance between monitoring and spontaneous retrieval more towards engagement of monitoring processes. Consequently, here participants might have adopted a strategy of monitoring for PM cues throughout the whole experiment, even though the remaining features of the PM tasks rendered this unlikely.

An alternative, not mutually exclusive, explanation of this observation builds on the notion of a more abstract general deviant search set that was outlined in Study 2 and a previous study (Walser et al., 2017). In brief, the idea is that forming an intention that requires detecting a particular stimulus establishes an attentional search set for that specific stimulus or a particular class of stimuli. Additionally, it also establishes a more generalized sensitivity towards deviance or a search set for any stimuli that deviate from the majority of stimuli presented in a task. Following this notion, ongoing-task costs in the focal condition in Study 2 would then not be the result of monitoring for specific PM cues, but stem from a more general bias to scan stimuli for deviance. If indeed forming an event-based intention established a general sensitivity towards deviance, then ongoing-task performance in deviant oddball trials that never served as a PM cue should also differ between conditions in which participants are instructed to perform a PM task and in ongoing-task only conditions in which participants had not performed a PM task beforehand. Although Walser et al. (2017) reported preliminary evidence for this, future research would be required to determine the validity of this notion.

Lastly, several views of PM functioning implicitly or explicitly assume that ongoing-task costs in PM paradigms arise due to the sharing of limited resources or capacities between a PM task and an ongoing task at hand (e.g., Guynn, 2003; McDaniel & Einstein, 2000; R. E. Smith, 2010). While the present line of studies does not allow to discern the source of ongoing-task costs, at least in parts, my findings seem to support the notion of a capacity sharing between PM and ongoing-task performance. Specifically, Study 3 showed that a high demanding ongoing 2-back working memory task impaired PM performance (lower accuracy and slower PM responses) compared to a low-demanding 1-back task, which could be explained by ongoing-task processes usurping resources away from PM performance. While such an explanation may seem plausible, note that capacity- or resource-sharing accounts of performance costs in general have been criticized for their potential circularity and limited explanatory value (e.g., Navon, 1984). Similarly, the capacity-sharing account of ongoing-task costs in PM is not undisputed either. While some recent studies suggest that capacity sharing might occur under particularly high task demands (Boag, Strickland, Heathcote, Neal, & Loft, 2019; Strickland et al., 2019), several others argue that many findings of ongoing-task costs in PM can instead be explained by altered response thresholds in lieu of capacity sharing (e.g., Ball & Aschenbrenner, 2018; Strickland, Loft, Remington, & Heathcote, 2018; cf. Anderson et al., 2018). These studies demonstrate that PM research is aware of the limitations of capacity-sharing accounts and is actively testing alternatives. At the same time, they also show that there is still a lot to do in order to elucidate the validity of the capacity-sharing notion. Most importantly for this, future research would be required that determines the specific type(s) of resource(s) shared between PM and ongoing-task performance, the conditions under which these are shared and the mechanisms through which such capacity sharing may occur.

6.4.3. Does PM Rely on Working Memory?

One provocative reading of the lack of acute stress effects on event-based PM performance that I observed in Studies 2 and 3 concerns the relation between PM and working memory that is assumed by a popular process model of PM (e.g., Kliegel et al., 2002; Kliegel, McDaniel, et al., 2008). The argument is that if acute stress impairs working memory (e.g., Schoofs et al., 2009), but not PM performance, then PM performance cannot rely on working memory. Indeed, this notion is supported by findings of impaired PM performance but preserved working memory in participants with neurological impairments (e.g., Carlesimo et al., 2011, 2014; Kamminga, O'Callaghan, Hodges, & Irish, 2014) or after transcranial magnetic stimulation to the dorsolateral prefrontal cortex (Basso, Ferrari, & Palladino, 2010). It is at odds, however, with findings of impaired PM performance under high working memory demands (e.g., Kidder et al., 1997; Logie, Maylor, Sala, & Smith, 2004; Marsh & Hicks, 1998; Meier & Zimmermann, 2015, see also Study 3 [Chapter 5] in this thesis) and associations between working-memory capacity and PM performance (e.g., Cherry & LeCompte, 1999; Reese & Cherry, 2002; Rose, Rendell, McDaniel, Aberle, & Kliegel, 2010; Schnitzspahn et al., 2013; cf. Brandimonte & Passolunghi, 1994; Breneiser & McDaniel, 2006).

Importantly, based on the present research, it is not possible to determine the relation between working memory and PM. For this, future research would be necessary to determine (a) whether the relationship between working memory and PM is mediated by one or more cognitive function(s) that themselves are not affected by acute stress and (b) whether the observed effects of stress on PM—reduced monitoring costs (Studies 2 & 3), increased time monitoring (Nater et al., 2006), faster intention retrieval (Szőllősi et al., 2018), impaired activity-based PM performance (Cuttler et al., 2019) and stable event-based PM performance 50+ min after stress induction (Glienke & Piefke, 2016)—can be explained by acute stress effects that do not primarily affect prefrontal-cortex functioning.

6.5. Limitations

Given the complexity of volitional action in general and PM and intention deactivation in particular, aiming to identify mechanisms and modulators of these functions through experimental investigations naturally requires limiting the scope of research questions and methodology on the one hand and limiting the applicability and generalizability of their findings and conclusions on the other hand. Consequently, the research reported in this thesis represents a snapshot of research questions, experimental designs and methods, rather than the complete picture; it is subject to limitations that I will summarize in the following.

First, besides a small sample of older adults in Study 1 (N = 24, age: 57–75 years), participants in the studies presented here were mostly younger university students (N = 232, age: 18–30 years) who volunteered in exchange for course credit or financial compensation. Hence, based on this research alone, it is not possible to assert to what extent my findings apply to the general population.

Second, while the simplified PM tasks in my experiments allowed to minimize the influence of confounding variables, like task-unrelated distractions (e.g., Harrison et al., 2014), they differ from PM tasks in everyday life in which we might, for instance, pursue multiple intentions at the same time and set external reminders for them ourselves. Consequently, although this approach provided good experimental control, limited the ecological validity of my research and its findings. As a compromise, future studies could, for instance, use paradigms that more realistically simulate the concurrent maintenance of multiple PM tasks over the course of a day (e.g., Glienke & Piefke, 2016) or the step-by-step nature of everyday errands (e.g., Rendell & Craik, 2000) and incorporate the option to self-set reminders for them (e.g., Gilbert, 2015). At least on the surface these procedures convey a higher ecological validity. Such research could then also be supplemented with data about everyday prospective remembering and the processes involved in it from experience sampling (e.g., Anderson & McDaniel, 2019).

Third, since my experiments exclusively used event-based PM tasks, they provide no information about the deactivation of time-based intentions or acute stress effects on time-based PM. This would be an interesting avenue for future research, since dissociations of brain-lesion effects on time-based but not event-based PM (e.g., Carlesimo et al., 2014; Volle et al., 2011) suggest that acute stress might indeed affect these types of PM tasks differently. Assessing the deactivation of time-based intentions, however, might prove rather difficult, since time-based PM strongly relies on self-initiated intention retrieval and $\mathrm{PM}_{\mathrm{REPEATED}}$ cues may not be particularly salient (e.g., the passing of another 5 min). This renders the occurrence of RT aftereffects and commission errors rather unlikely. Nevertheless, time-based PM tasks might incur aftereffects in terms of continued clock checking or rather subtle modulations of task performance following intention completion.

Fourth, the PM tasks in this thesis differ in important ways from typical PM paradigms (e.g., Einstein & McDaniel, 1990) and other approaches to assess intention deactivation. The paradigms in Studies 1 and 2 presented error feedback throughout the whole task—even for PM-response omissions or commission errors. While this was done to avoid artificially inflating aftereffects due to a lack of PM performance (Bugg & Scullin, 2013; Bugg et al., 2016), it deviates from the typical modus operandi of PM research (Brandimonte et al., 2014; Kliegel, McDaniel, et al., 2008; McDaniel & Einstein, 2007). As I discussed in Study 2, error feedback for commission errors in particular might foster a quick task-set reconfiguration through trial-and-error learning (e.g., Jueptner, Frith, Brooks, Frackowiak, & Passingham, 1997). Consequently, it might prompt participants who made a commission error to quickly avoid doing so in subsequent $\mathrm{PM}_{\mathrm{REPEATED}}$ trials. Also deviating from other aftereffect research (e.g., Scullin et al., 2012), participants in my experiments always performed multiple cycles of a PM task and a subsequent aftereffect measurement and were made aware of this prior to testing. It is therefore unclear to what extent my findings would be replicable in single cycle experiments. Previous studies that used this approach, however, suggested that effects on PM and aftereffects averaged across all cycles of an experimental session are also present during and after the first cycle (Walser et al., 2012; Walser, Plessow, et al., 2014).

Lastly, the studies I presented here, mostly provide information about a less drastic form of aftereffects—slowed ongoing-task responding. This response slowing presumably occurs due to altered decision-making or response-selection processes when completed intentions are retrieved during encounters of no-longer-relevant PM cues. The low number of commission errors across studies and, similarly, the almost absent output-monitoring errors in Study 3 suggest that I might have captured these functions at a rather low bound of demands. Hence, future research would be required to determine the effects of potential modulators of intention deactivation under higher demands. That is, when commission and output-monitoring errors might be more likely. Note, that this limitation in the past has proven rather difficult to circumvent, as one cannot build these errors into an experiment. What one can do, for instance, is design conditions that increase the likelihood for the occurrence of these types of errors, and hope. However, even this does not guarantee that commission errors occur in sufficient numbers (Anderson & Einstein, 2017).

Although these limitations do not undermine the validity of the research presented in this thesis, one should be aware that they limit the generalizability of its conclusions. At the same time, however, some of these limitations also point towards potential avenues for future research that I will outline in the following section.

6.6. Outlook

The present research raises a number of questions and open issues. Tackling these could not only further our understanding of prospective remembering and intention deactivation, but also in a broader sense might inform mechanisms and modulators of goal-directed, yet flexible behavior.

6.6.1. Mechanisms Behind Aftereffects of Completed Intentions[15]

First, one of the most pressing issues for future research on aftereffects of completed intentions is to reconcile the seemingly incompatible results from different strands of research. Specifically, it is vital to determine whether findings of intention inhibition in studies that assessed memory accessibility of the semantic content of intention representations (e.g., Marsh et al., 1998) or of semantic associates of PM cues (e.g., Förster et al., 2005) indeed reflect reduced accessibility of intention-related semantic memory contents or whether they rather reflect interference from continued intention retrieval. Such research would benefit strongly from the development of methods that allow testing the activation dynamics of different components of intention representations. Future experiments could, for instance, incorporate task features of both script-based paradigms and event-based PM paradigms to assess memory accessibility and retrieval of both the content of completed intentions and PM-cue–action associations. Additionally, while neuroimaging research on aftereffects is rare (Beck et al., 2014), neuroimaging studies of completed action representations might help to dissociate between inhibition and continued retrieval or residual memory activation of the content of completed intentions. For instance, sustained PM-task-related brain activation or the continued possibility to

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[15] Parts of this section (except for paragraph five) were adapted from the manuscript Chapter 2 is based on.

decode the PM-task set after intention completion would suggest a residual activation of a completed intention (see e.g., Momennejad & Haynes, 2013). Relatedly, determining whether and to what extent activation levels of intention representations after intention completion are related to the occurrence and size of aftereffects would enable us to determine whether a residual activation of completed intentions is a precondition or a mediator of RT aftereffects and commission errors, as proposed by the residual activation view I outlined in Chapter 2 (see also, Walser et al., 2012). If so, residual activation might increase the likelihood or readiness of spontaneous intention retrieval, as has been reported for uncompleted intentions (Schult & Steffens, 2017; see also Sugimori & Kusumi, 2008), which in turn could increase the risk of making a commission error after intention completion.

Second, with respect to findings of intention inhibition, it seems necessary to clarify under which conditions intention-related memory activation may drop below a baseline activation level, when this inhibition may be released and under which conditions activation levels simply decrease after compared to before intention completion. This would help to specify whether and when intention inhibition works like “intention suppression” and when it may work more like an active deactivation process that reduces intention activation towards a neutral baseline.

Third, regarding the involvement of cognitive control in intention deactivation, future research would be necessary to determine (a) the type of cognitive control functions that modulate intention deactivation (e.g., response inhibition vs. selective attention), (b) the relative involvement of cognitive control over intention execution or response selection in intention deactivation, (c) whether it is possible to control spontaneous intention retrieval itself and (d) to what extent we can proactively mobilize control over these processes after intention completion. This would help to identify conditions in which aftereffects might become particularly problematic and also help to develop strategies to aid rapid intention deactivation.

Fourth, research on aftereffects in event-based PM paradigms yielded aftereffects in several different dependent variables, like RTs, commission-error risk, commission-error rates and thoughts about the completed intention. To date it is unclear in what ways these measures relate to each other and whether aftereffects in different dependent variables result from the same or from different underlying processes. For instance, Anderson and Einstein (2017) argued that the absence of commission errors does not necessarily reflect a direct deactivation of an intention representation. Instead, residual intention activation in combination with good task understanding or intact cognitive control might also lead to the absence of commission errors. Hence, future research might benefit from adding thought probes and/or RT measures of aftereffects in $\mathrm{PM}_{\mathrm{REPEATED}}$ trials as dependent variables since these measures might be more sensitive to capture aftereffects in the absence of commission errors.

Fifth, in Chapter 2 I suggested that the seemingly contradictory findings from different lines of aftereffects research could be explained by the fact they focus on different elements of intention representations that might exhibit different deactivation dynamics (i.e., the semantic content vs. PM-cue–action associations). While this seems plausible, the divergence of findings could also be an artifact or side effect of the experimental procedures. Specifically, the occurrence of intention inhibition or continued intention retrieval might depend upon whether or not it is possible to perform a no-longer-relevant intention within the confines of the aftereffects measurement or whether this possibility is particularly salient. To test this, future studies could, for instance, have participants postpone performance of (parts of) a complex action script or a series of actions *within* the context of an ongoing task in response to a PM cue. Then, after intention completion, script words, semantic PM-cue associates and $\mathrm{PM}_{\mathrm{REPEATED}}$ cues should incur RT aftereffects or produce commission errors. Conversely, moving performance of a simple PM response into another context *outside* of a PM task or ongoing task should produce intention inhibition or should cease to produce aftereffects when $\mathrm{PM}_{\mathrm{REPEATED}}$ cues are presented after intention completion. Unfortunately, the one study in which semantic associates of a PM cue could have prompted participants to perform (parts of) a completed intention (i.e., stopping the experiment and notifying the experimenter) and which could thus have provided preliminary information about this, did not report data on commission errors (Förster et al., 2005). However, it seems at least feasible that the semantic content of an intention could trigger intention retrieval, since semantic associates of PM cues have been shown to trigger actual intention retrieval in event-based PM tasks (Cook et al., 2006; cf. Mullet et al., 2013).

Last but not least, although there are some reports of commission errors in everyday life, such as accidental overmedication (e.g., Kimmel et al., 2007), the prevalence of commission errors and their potentially negative consequences in real life are widely unknown—especially for potential high risk groups such as older adults. Future studies could answer these questions, for instance, by using experience sampling of real-life prospective memory failures and aftereffects of completed intentions (e.g., Anderson & McDaniel, 2019; Krönke et al., 2018) or extend existing self-report measures of PM to include intention-deactivation failures (e.g., the *Prospective and Retrospective Memory Questionnaire*; G. Smith et al., 2000). Based on intuition, commission errors seem to be (way) less common than forgetting to perform an intended action (omission errors). How and why did the cognitive system evolve to minimize commission errors even if this may increase the likelihood of omission errors? Or put slightly differently, did the cognitive system adapt over time to avoid some omission errors by allowing for some commission errors? One might also ask whether this relation may change dynamically, for instance, depending on situational demands or meta-control parameters that modulate the dynamic interplay between PM performance and intention deactivation.

6.6.2. Routes of Action for Acute Stress Effects on PM

In a recent meta-analytic integration of three studies that tested effects of acute stress on PM, Piefke and Glienke (2017) showed that, on average, acute stress produced a small improvement of PM-cue detection in event-based PM tasks. However, they also showed that there is considerable heterogeneity across studies. The authors speculated that this heterogeneity could be attributed to differences between PM tasks and the use of different stress-induction protocols in these studies. Although I found that the presence of acute stress effects on *PM monitoring* and intention deactivation can be affected by PM-task features (Study 2 focal vs. nonfocal condition), it seems unlikely that differences in PM-task features in typical laboratory PM tasks alone can account for the heterogeneity of stress effects on *PM performance* in previous studies. This appears particularly unlikely, since PM-task features did not influence the occurrence of acute stress effects in the present studies. For instance, I found that the absence of acute stress effects on PM performance did not depend on prospective load and PM-cue focality (Study 2) or on ongoing-task demands (Study 3). Moreover, Szőllősi et al. (2018) recently found that acute stress induction speeded up PM responses in a task that—similar to Studies 2 and 3 in this thesis—used nonsalient and nonfocal PM cues.

By contrast, different stress-induction protocols indeed seem to have different effects on PM. While exposure to the Socially evaluated cold pressor test (SECPT) led to stable accuracy of PM performance over time (Glienke & Piefke, 2016) and increased the speed of PM responses (Szőllősi et al., 2018), exposure to the MAST (Study 3) or the TSST did not (Möschl et al., 2017 [Study 2 in this thesis]; Nater et al., 2006; Walser et al., 2013). However, a recent study also showed that MAST exposure impaired PM performance in an activity-based task that required participants to rate their pain levels after each of the five hand immersions into ice-cold water during the MAST (Cuttler et al., 2019). As I suggested in Chapter 5, a critical difference between these studies lies in the strength and types of physiological stress responses that are associated with the stress-induction protocols they used. Specifically, both the TSST and the MAST are associated with a larger cortisol response than the SECPT (Schwabe & Schächinger, 2018; Smeets et al., 2012). Hence, stress effects on PM performance might primarily be mediated by cortisol responses that counteract residual stress effects (e.g., Hermans et al., 2014) which are themselves differentially related to stress effects on cognition (Schönfeld, Ackermann, & Schwabe, 2014). On speculative terms, cortisol levels and PM functioning might even exhibit a dose–response relation as has been observed for example in declarative memory (Schilling et al., 2013) and working memory (Lupien, Gillin, & Hauger, 1999). Although a first reanalysis of data points towards a modulation of acute-stress effects on PM by cortisol in high versus low responders (Glienke & Piefke, 2017), the correlative nature of such findings so far only allows speculations about the relative roles of cortisol secretion and other residual stress effects in the genesis of acute stress effects on PM.

To determine the causal roles of cortisol and residual stress responses for acute stress effects on PM, one could systematically test the effects of cortisol responses with minimal conflation from residual stress effects through direct administration of hydrocortisone (e.g., Miller et al., 2015; Weckesser et al., 2016). Conversely, one could test the effects of residual stress effects with minimal conflation from cortisol responses by applying different stress-induction protocols in conjunction with administration of dexamethasone or prednisolone that have been shown to considerably reduce cortisol secretion (Ali, Nitschke, Cooperman, & Pruessner, 2017; Pariante et al., 2004; Rimmele, Meier, Lange, & Born, 2010; cf. Kirschbaum & Hellhammer, 1994). Interestingly, while Ballhausen, Kliegel, and Rimmele (2019) most recently, found no effects of hydrocortisone administration on PM, their exploratory analyses showed that PM performance accuracy and task-demand effects on PM monitoring (focal vs. nonfocal PM cues) seemed to be greater when hydrocortisone or placebo pills were administered at 3 p.m. than at 1 p.m. Consequently, future studies on this should also take into account the circadian variability of PM functioning (Rothen & Meier, 2017). In order to more clearly elucidate the relative roles of stress-related neurochemical changes in the genesis of stress effects on cognitive functioning, such an approach could also be extended towards testing effects of selective blockage of mineralocorticoid receptors (Cornelisse, Joëls, & Smeets, 2011; Otte et al., 2007; Schwabe et al., 2013) or glucocorticoid receptors (Furay, Bruestle, & Herman, 2008; Mailliet et al., 2008; Yuen et al., 2011). On a broader scope, this may ultimately also help inform potential routes of action to alleviate symptoms of stress-related disorders like Depression (e.g., Iwata, Ota, & Duman, 2013) or post-traumatic stress disorder (e.g., Pitman et al., 2012).

6.6.3. "…isn't this the same?" Aftereffects Outside of PM[16]

An issue that goes beyond the scope of the present thesis, but is interesting from a theoretical perspective, is the relation of aftereffects of completed intentions to other research areas such as task switching (e.g., Kiesel et al., 2010) or instruction-based learning (e.g., Meiran et al., 2012). Here, some task-switching studies reported so-called *N*-2 repetition costs that may be viewed as similar to aftereffects of completed intentions. This so-called backward-inhibition effect refers to the finding that switching to a previously relevant task set impairs task performance more than switching to a novel task set. Interestingly, both *N*-2 repetition costs and findings of slowed lexical decisions on intention-related words after intention completion in script-based paradigms have been explained by a common mechanism—inhibition of previously relevant task sets (e.g., Koch, Gade, Schuch, & Philipp, 2010). However, similar to aftereffects in event-based PM tasks, *N*-2 repetition costs have also been theorized to stem from continued retrieval of a previously relevant task set. That is, the re-activated *N*-2 task set is thought to interfere with the still residually active *N*-1 task set that was just switched from (e.g., MacLeod, 2007; MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003).

Relatedly, research on instruction-based learning showed that instructed stimulus–response rules or mappings incur costs in terms of stimulus- or response-congruency effects even when stimuli that are specified in these mappings occur in a task-irrelevant context or dimension (e.g., Liefooghe, Degryse, & Theeuwes, 2016; Meiran et al., 2012). This research also showed that binding instructed stimulus-response mappings to specific task contexts requires actual performance or prior practice (Braem, Liefooghe, De Houwer, Brass, & Abrahamse, 2017). These findings may be viewed as similar to findings from

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[16] The first paragraph of this section was adapted from the manuscript Chapter 2 is based on.

research in event-based PM paradigms. For instance, $\mathrm{PM}_{\text{REPEATED}}$ cues have also been shown to elicit intention retrieval in contexts in which an intention is no-longer relevant (e.g., Anderson & Einstein, 2017; Walser et al., 2012). Additionally, commission errors have been shown to occur more frequently when PM-task performance had been cancelled or was not possible (due to the absence of PM cues) than after successful PM-task performance (Bugg & Scullin, 2013; Bugg et al., 2016; Schaper & Grundgeiger, 2017). However, the absence of commission errors after a change in the ongoing task (Scullin et al., 2012) suggests that aftereffects may be more context specific and less rigid than instructed stimulus–response mappings.

In summary, although all of these research areas assess effects of instructed stimulus–response associations and some effects reported by them appear similar, there are still substantial differences between paradigms. Hence, further research would be necessary to test whether and to what extent these phenomena indeed reflect similar mechanisms.

6.7. Conclusion

Overall, the research presented in this thesis contributes towards systematizing and understanding the processes underlying aftereffects of completed intentions as well as the circumstances and factors that influence prospective remembering and intention deactivation as two central capacities of volitional action. My synopsis of research on intention deactivation, showed that although completed intentions are not always immediately and fully deactivated, they are also not always continually retrieved either. Additionally, Chapter 3 showed that even though cognitive-control abilities seem to play a role for intention deactivation, more work is required to elucidate the conditions under which cognitive-control abilities come into play and which specific aspects of cognitive-control abilities are crucial for intention deactivation. More importantly, however, my research suggests that intention deactivation should be conceptualized as a dynamic process that moves along a continuum from complete re-activation to complete de-activation of intentions that is shifted by factors that also influence the retrieval of active intentions. Additionally, Chapters 4 and 5 showed that, contrary to intuition and research on higher-order cognitive functions, acute stress does not seem to impair prospective remembering when it can rely on external reminders. Instead, it seems to actually make pursuit of such event-based intentions less costly and perhaps more efficient. Importantly, here future research on the relative influence of different physiological stress responses could substantially further our understanding of the routes of actions and the neurochemical changes through which acute stress affects cognitive functioning. Overall, as I hinted on in the general discussion, it could be worthwhile for research on PM and intention deactivation, on the one hand, to broaden its scope and consider conceptually related phenomena in other research areas, and, on the other hand, to take a closer look at the underlying mechanisms through which modulating factors exert their influence on prospective remembering and intention deactivation. Lastly, I hope that the research and the open issues I presented here can stimulate future research that—together with my findings—could further elucidate the role of intentions for adaptive goal-directed behavior and may also advance psychology's "journey" towards a more complete understanding of volition and cognitive control.

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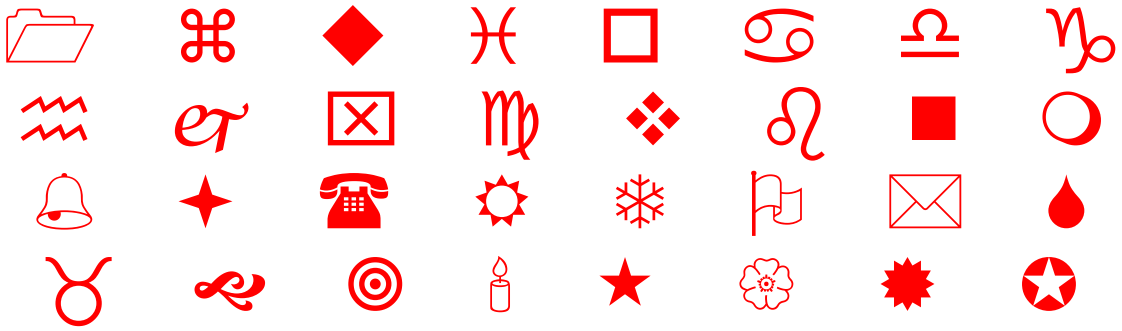
8. Appendix A

Stimulus Set for PM and Oddball Cues (Chapter 3)



9. Appendix B

Stimulus Set for the Cognitive Task (Chapter 4)

Table B1

PM-cue and distractor words in the nonfocal condition (verbs on white, nouns on grey background).

PM-cue words	Distractor words
TRA	
TRAMPELN, BEANTRAGEN	TRÖSTEN, TRIEFEN, TRÖDELN, BETRÜGEN, STRÄUBEN, ENTROLLEN, ABTROPFEN, TROMPETEN, STRIEGELN, ENTROSTEN, ENTRICHTEN, ENTRÜMPELN, ENTRÄTSELN, ENTRIEGELN, AUFTRUMPFEN
WELTRAUM, STRAND	STROM, STROH, TRUHE, STRICH, NOTRUF, BISTRO, TRIOLE, BETRIEB, ZENTRUM, GETRÄNK, TRIUMPH, STROPHE, ZITRONE, NEUTRON, KONTROLLE
SPU	
SPUKEN, ABSPULEN	SPRITZEN, SPITZEN, SPIELEN, SPIEGELN, SPANNEN, SPALTEN, SPÄHEN, ENTSPANNEN, DURCHSPIELEN, BESPRÜHEN, BESPAßEN, BEANSPRUCHEN, AUFSPIESSEN, ANSPORNEN, ABSPALTEN
FAHRSPUR, DISPUT	ASPHALT, BEISPIEL, DISPOSITION, FUNKSPRUCH, GASPEDAL, GESPENST, GRÜNSPECHT, HÖRSPIEL, KNOSPE, ORGANSPENDE, PROSPEKT, SCHAFSPELZ, SPITZENKOCH, SPONSOR, VORGESPRÄCH
FLI	
AUFFLIEGEN, FLITZEN	SCHIEFLAUFEN, PFLÜGEN, PFLEGEN, PFLANZEN, FLUTEN, FLÜCHTEN, FLUCHEN, ENTFLECHTEN, BEPFLANZEN, AUFLÖSEN, AUFLEUCHTEN, AUFLESEN, AUFLEHNEN, AUFLEGEN, ANFLEHEN
KONFLIKT, SCHULPFLICHT	ZAHNFLEISCH, TEFLON, SOUFFLE, PFLUG, NEUAUFLAGE, GRÜNFLÄCHE, FLUTWELLE, FLUGZEUG, FLOTTE, FLASCHE, FLADENBROT, DORFLADEN, DAMPFLOK, BAUFLÄCHE, AUFLAUF
ERM	
VERMIETEN, ERMUNTERN	WERFEN, ÄUßERN, RUDERN, POKERN, ERHÖHEN, FÖRDERN, WANDERN, ERFORSCHEN, WIDERRUFEN, ERSIEGELN, SCHNEIDERN, HERUMTOBEN, WEITERLEITEN, VERSCHROTTEN, DURCHLÖCHERN
ÜBERMUT, SONDERMÜLL	SERIE, FEUER, STERN, ERFOLG, HERBST, PAPIER, GERICHT, VERBAND, PFEFFER, HERBERGE, NUTZTIER, SERVIETTE, SÜßWASSER, LADEGERÄT, EISBECHER
ACK	
ANPACKEN, WACKELN	ZUMACHEN, WETTMACHEN, WACHRÜTTELN, PACHTEN, NACHWEISEN, NACHGEBEN, NACHDENKEN, MITMACHEN, LACHEN, FESTMACHEN, COACHEN, BEWACHEN, BEOBACHTEN, ACHTGEBEN, ABMACHEN,
JACKE, SACKGASSE	WACHSTUM, WACHHUND, SEGELYACHT, SCHIEBEDACH, SCHACHKLUB, SACHMITTEL, SACHBUCH, NACHTLEBEN, MACHT, LACTOSE, FACHWISSEN, EISFACH, DACHS, ACRYL, ACHSE
ING	
EINGEBEN, SINGEN	WEINEN, PINSELN, HINSETZEN, HINLEGEN, HINKEN, GRINSEN, GEWINNEN, FESTBINDEN, EINPRÄGEN, EINÖLEN, EINMISCHEN, EINLADEN, EINFÜGEN, ANLEINEN, ABWINKEN
DOPING, WEINGLAS	WINDBÖE, WEINFEST, VINYL, URINSTINKT, STEIN, RUINE, PROVINZ, MINUTE, MAGAZIN, LEINWAND, KINDHEIT, INNENHOF, FEINUNZE, EINZUG, BENZIN

Table B2

PM-cue and oddball words in the focal condition (verbs on white, nouns on grey background). Each oddball word was matched to a corresponding PM-cue word in regard to word type, frequency and length.

PM cues	Oddball cues
ABSAGEN, AUFSCHEUCHEN, BENEIDEN, DURCHKNETEN, ENTSCHÄRFEN, HANDELN, KÄMPFEN, MÖGEN, SCHMUNZELN, UMKIPPEN	STÜTZEN, DURCHSTECHEN, UMDENKEN, ANSCHWEIßEN, BEIBEHALTEN, ZÄHLEN, BEWEGEN, LEBEN, UMBENENNEN, ABFÜLLEN
AUTOKLUB, BÜFFEL, FUßBANK, HANDSCHUH, MITTE, OZONLOCH, ROLLE, SENFKORN, TORSCHÜTZE, WALDSEE	BAUWAGEN, KNÖDEL, OHRLOCH, TURBULENZ, KUNST, NEUKUNDE, KRIEG, BLUTEGEL, HAFTBEFEHL, SCHÜRZE

Table B3

Standard words for the initial task practice (verbs on white, nouns on grey background).

ABLEHNEN, ABNUTZEN, ANDÜNSTEN, BEFEUCHTEN, BEFÜLLEN, BESCHMUTZEN, BEVORZUGEN, ENTGLEISEN, FESTHALTEN, GUTHEIßEN, HALTEN, KLEIDEN, KOSTEN, MITWIRKEN, NAHESTEHEN, PADDELN, PROPHEZEIEN, SEGELN, STECHEN, WEGLAUFEN
ALBUM, APRIKOSE, BIOMASSE, BLUTPROBE, BROTKORB, BÜRSTE, FESTLAND, GEWICHT, HALSKETTE, HOROSKOP, KOMÖDIE, LAUFZEIT, LUFTPOST, MUNDLOCH, NUTZLAST, ROMAN, SCHNEIDEZAHN, TEELÖFFEL, TORNADO, UNWISSEN

Table B4

Standard words in the cognitive task (verbs on white, nouns on grey background).

ABBAUEN, ABBESTELLEN, ABBEZAHLEN, ABBUCHEN, ABGEBEN, ABGEWÖHNEN, ABHALTEN, ABHEBEN, ABHOLEN, ABKLOPFEN, ABLENKEN, ABLESEN, ABMÄHEN, ABMALEN, ABRIEGELN, ABROLLEN, ABRUTSCHEN, ABSCHALTEN, ABSCHÄTZEN, ABSCHLEPPEN, ABSCHÜTTELN, ABSCHWÄCHEN, ABSTÜRZEN, ABWÄGEN, ABZAHLEN, ANBAUEN, ANBEIßEN, ANFECHTEN, ANHABEN, ANHEBEN, ANKNÜPFEN, ANKOPPELN, ANORDNEN, ANPASSEN, ANRUFEN, ANSAGEN, ANSCHAFFEN, ANSETZEN, ANWEISEN, ANWENDEN, ANZAPFEN, ANZETTELN, ÄTZEN, AUFBIEGEN, AUFDRÖSELN, AUFFÄDELN, AUFFALLEN, AUFHALTEN, AUFHORCHEN, AUFMÖBELN, AUFRUFEN, AUFSCHLAGEN, AUFSCHÜTTEN, AUFSTEHEN, AUFSTELLEN, AUFWEICHEN, AUFWIRBELN, AUFZÄHLEN, AUFZEICHNEN, BADEN, BAUEN, BEDANKEN, BEGLEICHEN, BEGRÜNDEN, BEGRÜNEN, BEHALTEN, BEHANDELN, BEICHTEN, BEIMISCHEN, BEKLEBEN, BEKOCHEN, BELASTEN, BELEUCHTEN, BENUTZEN, BESCHENKEN, BESIEGELN, BESTÄRKEN, BESTELLEN, BEZAHLEN, BEZIEHEN, BIETEN, BITTEN, BOXEN, BRÄUNEN, BRÜLLEN, BRÜTEN, BÜNDELN, DABEIBLEIBEN, DAZUGEBEN, DAZULEGEN, DEHNEN, DIENEN, DURCHDENKEN, DURCHLESEN, DURCHSETZEN, EKELN, ENTFALTEN, ENTFRISTEN, ENTHÄUTEN, ENTKORKEN, ENTLASSEN, ENTSORGEN, ENTWENDEN, ENTWURZELN, ENTZÜNDEN, ESSEN, FÄDELN, FÄRBEN, FESSELN, FESTLEGEN, FESTNAGELN, FESTSETZEN, FOLGEN, FRÖSTELN, FÜHLEN, GEBEN, GEGENLESEN, GEHEIMHALTEN, GEWICHTEN, GLÄTTEN, GROßZIEHEN, HEIZEN, HOCHGEHEN, HOCHKLAPPEN, HOCHSCHIEBEN, HOCHZIEHEN, HOCHZÜCHTEN, HOFFEN, HOLEN, HUMPELN, HÜPFEN, KIPPEN, KLOPFEN, KNOBELN, KÜHLEN, KUNDTUN, KÜRZEN, KÜSSEN, LANDEN, LENKEN, LESEN, LOBEN, LÖFFELN, LÖSCHEN, LOSGEHEN, LOSLÖSEN, MÄHEN, MALEN, MIETEN, MITFÜHLEN, ENTWISCHEN, MITNASCHEN, MUNKELN, MUTMAßEN, NASCHEN, NENNEN, OFFENLEGEN, ÖFFNEN, QUIETSCHEN, RÄTSELN, RIESELN, RUFEN, RUMKRIEGEN, RUMSITZEN, RUTSCHEN, SAGEN, SÄGEN, SCHALTEN, SCHÄTZEN, SCHENKEN, SCHIELEN, SCHIMPFEN, SCHLAFEN, SCHLEICHEN, SCHLÜRFEN, SCHMIEDEN, SCHMUGGELN, SCHNAPPEN, SCHNAUFEN, SCHNEIDEN, SCHRUMPFEN, SCHUFTEN, SCHUNKELN, SCHWEBEN, SCHWITZEN, SEHEN, SITZEN, STÜRZEN, TÄTSCHELN, TOASTEN, UMBAUEN, UMJUBELN, UMLEITEN, UMSCHALTEN, UMSCHWENKEN, UMSORGEN, UMWÄLZEN, UMWÜHLEN, VOLLLADEN, VORBESTELLEN, VORBEUGEN, VORFÜHLEN, VORKOSTEN, VORLADEN, VORLASSEN, VORSCHLAGEN, VORSORGEN, VORSTELLEN, VORWASCHEN, VORZEICHNEN, WÄHLEN, WASCHEN, WECHSELN, WEGGEHEN, WEGLASSEN, WEGROLLEN, WEGSCHNAPPEN, WEISSAGEN, WIEGEN, WOHNEN, WÜHLEN, WÜNSCHEN, WÜRFELN, WÜRZEN, ZEICHNEN, ZIEHEN, ZÜCHTEN, ZUGEBEN, ZUKNÖPFEN, ZUWEISEN
ABEND, ABGASWOLKE, ABSCHAUM, ABSOLVENT, AGENTUR, ALKOHOL, ALPHABET, AMSEL, AMTSZEIT, ANEKDOTE, APFELSCHORLE, APOTHEKE, AQUÄDUKT, ASTLOCH, AUFBRUCH, AUFSCHUB, AUGAPFEL, AUTOBAHN, AUTOHANDEL, AUTOR, AUTORITÄT, AUTOSITZ, BADEGAST, BAHNHOF, BALLSAAL, BALSAM, BANDMAß, BASIS, BASKETBALL, BAUKLOTZ, BELEGSCHAFT, BESCHEID, BETONBAU, BETONKLOTZ, BIOGASANLAGE, BIOLADEN, BIOTONNE, BIRNBAUM, BLAUALGE, BLEICHMITTEL, BODEN, BODENBELAG, BORDEAUX, BRIEF, BÜHNE, BÜROGEBÄUDE, CHAMPION, CHANCE, COLLAGE, CURRY, DENKMAL, DETEKTEI, DETEKTIV, DIENSTWAGEN, DISKOTHEK, DIVISION, EDELHOLZ, EISDIELE, ELCHGEWEIH, ENTSCHEID, ENTWURF, ENZYKLOPÄDIE, EXKURSION, EXPLOSION, FAHRGAST, FAHRPLAN, FALLTÜR, FASSADE, FELDBETT, FELSWAND, FESTJAHR, FESTNETZ, FOLGE, FRIEDHOF, FRISBEE, FRÜHJAHR, FUNKRUF, FUNKTION, FUNKTIONÄR, FUßBALL, GASMASKE, GEBIET, GEDICHT, GEGENPOL, GEHÖRSTURZ, GEIST, GESCHENK, GESCHIRR, GESETZ, GROßHIRN, GRUND, GRUNDGESETZ, GUMMIKUGEL, GYMNASIUM, HALLENBAD, HANDBUCH, HANDTUCH, HIRNHAUT, HIRSE, HOCHSAISON, HOCHZEIT, HOLZFASS, HOLZFUßBODEN, HORIZONT, HÖRSAAL, HOTEL, HÜLSENFRUCHT, IDENTITÄT, IMPFPASS, IMPFSTOFF, JAHRBUCH, JUGENDAMT, KIRCHE, KNAUTSCHZONE, KOHLKOPF, KOMMUNALWAHL, KOMMUNE, KOMPASSNADEL, KOMPOST, KONFEKT, KONJUNKTUR, KOPFTUCH, KRISE, KRITIK, KÜCHENSTUDIO, KULTUR, KURHOTEL, LASTWAGEN, LAUBBAUM, LEBENSMITTEL, LEGENDE, LEHRPLAN, LEIHGABE, LEUCHTMITTEL, LEXIKON, LÖSEGELD, LUFTKORRIDOR, LUFTLOCH, MAGMA, MAHLZEIT, MAISKORN, MÄRCHEN, MAßBAND, MAULWURF, MIKROCHIP, MIKROFON, MODEWELT, MORPHIUM, MOTTO, NÄHZEUG, NEUWAGEN, NOTENBANK, NOTHALT, ÖKONOMIE, OPTIMUM, PANDABÄR, PASTETE, PEITSCHE, PETITION, PLANKTON, PLASTIKTÜTE, POTENZIAL, PROFESSOR, PROTOKOLL, PROTOTYP, PROZESS, QUELLE, RHETORIK, RHYTHMIK, ROLLSTUHL, ROTKOHL, RUHEPOL, RUNDE, RUNDFUNK, SAISON, SCHÄDELBRUCH, SCHALTJAHR, SCHLAGZEUG, SCHLAUCH, SCHLOSS, SCHLÜSSEL, SCHMIEDE, SCHNALLE, SCHNAUZE, SCHUBLADE, SCHULJAHR, SCHULNOTE, SCHÜSSEL, SCHWALBE, SCHWEFEL, SCHWIMMBAD, SEEWEG, SEITE, SEKUNDE, SELBSTKRITIK, SITZBANK, SITZHÖHE, SKIHÜTTE, SKULPTUR, SOFTEIS, SONNE, SORGE, STOFFWECHSEL, STUNDENPLAN, SYNTHESE, SZENE, TEDDYBÄR, TEEKÜCHE, TEEZEIT, TELEFON, THEORIE, TISCH, TITEL, TOURIST, TURNHALLE, VAGABUND, VOLLMOND, WAFFE, WAHLJAHR, WANZE, WASCHKÜCHE, WEBSEITE, WEISHEIT, WIRBELSÄULE, WISSENSCHAFT, WOCHE, WOHNSITZ, ZEITGEIST, ZUGVOGEL, ZÜNDHOLZ

Table B5

Color pairs for PM cues and oddballs in the focal condition (RGB values in brackets).

PM-cue color		Oddball color	
light blue	[11, 97, 164]	light orange	[255, 146, 0]
blue	[27, 27, 179]	ochre	[255, 191, 0]
purple	[169, 14, 255]	yellow	[255, 255, 0]
pink	[255, 0, 144]	yellow-green	[170, 255, 0]
red	[228, 0, 69]	green	[103, 227, 0]
orange	[255, 73, 0]	turquoise	[0, 175, 100]

10. Appendix C

Supplement and Stimulus Information (Chapter 5)

Table C1

Colors for PM cues in the cognitive task (RGB values in brackets).

	Cue color
brown	[140, 94, 24]
blue	[49, 2, 176]
green	[0, 176, 80]
orange	[255, 132, 0]
pink	[200, 54, 153]
purple	[113, 4, 168]
red	[203, 19, 5]
turquoise	[134, 204, 203]
yellow	[234, 209, 50]
yellow-green	[170, 255, 0]

Table C2

Random effects characteristics in the main analysis.

Analysis		***SD***	**Trials**
PM-cue identification			
RT	participants	0.229	2200
	residual	0.344	
Errors	participants	0.754	3064
Output monitoring			
RT	participants	0.230	2063
	residual	0.403	
Errors	participants	1.624	2175
Ongoing-task performance (PM monitoring)			
RT	participants	0.203	57093
	residual	0.315	
Errors	participants	0.555	63042

Note. All analyses were based on 79 participants.

Table C3

Regression of the primary performance measures on various baseline covariates and treatment contrasts based on the complete dataset.

	Correct response time (RT)				**Error probability (ERR)**			
Performance measure	***M*(B)**	***SE*(B)**	***t*-value**	***p*-value**	***M*(B)**	***SE*(B)**	***z*-value**	***p*-value**
PM-cue identification								
Intercept	6.376	0.031	208.435		−1.378	0.131	−10.549	
Cycle (1–10)	−0.025	0.003	−8.545		0.038	0.016	2.375	
PM cues per cycle (3–5)	0.009	0.012	0.739		−0.251	0.068	−3.692	
N-back match trial (yes/no)	−0.113	0.015	−7.322		−0.003	0.090	−0.037	
Ongoing-task demand: low vs. high (0/1)	0.103	0.019	5.505		0.733	0.110	6.640	
Treatment: no-stress vs. stress (0/1)	0.033	0.030	1.086	.278	−0.201	0.172	−1.170	.242
Treatment × Ongoing-task demand	−0.044	0.030	−1.443		0.223	0.175	1.273	
Output monitoring								
Intercept	6.972	0.033	214.482		−3.971	0.354	−11.232	
Cycle (1–10)	−0.054	0.004	−15.403		−0.046	0.045	−1.018	
PM cues per cycle (3–5)	−0.025	0.014	−1.746		0.408	0.193	2.115	
N-back match trial (yes/no)	0.001	0.019	0.070		−0.126	0.243	−0.521	
Ongoing-task demand: low vs. high (0/1)	0.046	0.023	2.010		−0.252	0.281	−0.899	
Treatment: no-stress vs. stress (0/1)	−0.020	0.036	−0.550	.582	−0.140	0.420	−0.334	.739
Treatment × Ongoing-task demand	−0.046	0.037	−1.247		−0.131	0.501	−0.261	
Ongoing-task performance (PM monitoring)								
Intercept	6.157	0.023	268.147		−3.390	0.087	−38.964	
Cycle (1–10)	−0.021	0.001	−41.267		−0.033	0.006	−5.975	
PM cues per cycle (3–5)	−0.002	0.002	−0.829		−0.096	0.023	−4.228	
N-back match trial (yes/no)	0.016	0.003	5.540		1.549	0.029	54.046	
Ongoing-task demand: low vs. high (0/1)	0.185	0.005	33.745		0.930	0.070	13.369	
Block: test block vs. PM block (0/1)	0.133	0.005	27.664		0.687	0.065	10.505	
Treatment: no-stress vs. stress (0/1)	−0.036	0.008	−4.723		−0.257	0.103	−2.483	
Ongoing-task demand × Block	−0.045	0.007	−6.506		−0.362	0.082	−4.442	
Treatment × Ongoing-task demand	0.003	0.009	0.306		0.163	0.114	1.429	
Treatment × Block	**−0.039**	**0.008**	**−5.108**	**< .001**	−0.120	0.110	−1.092	.275
Treatment × Ongoing-task demand × Block	0.024	0.011	2.182	.029	0.034	0.134	0.251	

Note. In this table, *p*-values are reported only for a-priori defined hypothesis tests that were conducted in the main analysis. Statistically significant treatment effects are printed in bold font (*p* < .005 [.03/6]; see second-level analyses).

Table C4

Exploratory regression of the primary performance measures on various baseline covariates and treatment contrasts over time.

	Correct response time (RT)				**Error probability (ERR)**			
Performance measure	***M*(B)**	***SE*(B)**	***t*-value**	***p*-value**	***M*(B)**	***SE*(B)**	***z*-value**	***p*-value**
PM-cue identification								
Intercept	6.560	0.035	185.132	< .001	−1.585	0.182	−8.700	< .001
PM cues per cycle (3 – 5)	0.009	0.012	0.731	.465	−0.248	0.069	−3.585	< .001
N-back match trial (yes/no)	−0.110	0.016	−6.950	< .001	0.014	0.092	0.149	.881
Cycle (1 – 10)	−0.030	0.005	−6.666	< .001	0.027	0.028	0.958	.338
Ongoing-task demand: low vs. high (0/1)	0.089	0.036	2.466	.014	0.515	0.216	2.385	.017
Treatment: no-stress vs. stress (0/1)	−0.060	0.057	−1.053	.292	0.016	0.367	0.043	.966
Cycle × Ongoing-task demand	0.004	0.006	0.619	.536	0.048	0.037	1.303	.193
Cycle × Treatment	0.017	0.008	2.007	.045	−0.023	0.055	−0.414	.679
Treatment × Ongoing-task demand	0.030	0.083	0.362	.717	0.648	0.480	1.349	.177
Cycle × Treatment × Ongoing-task demand	−0.013	0.013	−1.050	.294	−0.073	0.073	−1.004	.316
Output monitoring								
Intercept	7.354	0.039	189.939	< .001	−3.846	0.454	−8.468	< .001
PM cues per cycle (3 – 5)	−0.020	0.014	−1.392	.164	0.367	0.195	1.888	.059
N -back match trial (yes/no)	0.001	0.019	0.039	.969	−0.134	0.248	−0.540	.589
Cycle (1 – 10)	−0.063	0.005	−11.522	< .001	−0.017	0.069	−0.249	.803
Ongoing-task demand: low vs. high (0/1)	0.042	0.044	0.955	.340	−0.082	0.568	−0.144	.885
Treatment: no-stress vs. stress (0/1)	−0.096	0.068	−1.403	.161	0.145	0.840	0.172	.863
Cycle × Ongoing-task demand	0.001	0.008	0.098	.922	−0.019	0.098	−0.190	.849
Cycle × Treatment	0.018	0.001	1.736	.083	−0.047	0.128	−0.368	.713
Treatment × Ongoing-task demand	−0.182	0.100	−1.824	.068	−0.474	1.468	−0.323	.747
Cycle × Treatment × Ongoing-task demand	0.023	0.015	1.487	.137	0.012	0.223	0.052	.959
Ongoing-task performance (PM monitoring)								
Intercept	6.262	0.024	261.998	< .001				
PM cues per cycle (3 – 5)	−0.001	0.002	−0.649	.516				
N -back match trial (yes/no)	0.016	0.003	5.572	< .001				
Ongoing-task demand: low vs. high (0/1)	0.244	0.011	22.975	< .001				
Cycle (1 – 10)	−0.016	0.001	−11.972	< .001				
Block: test block vs. PM block (0/1)	0.179	0.009	19.525	< .001				
Treatment: no-stress vs. stress (0/1)	−0.065	0.017	−3.932	< .001				
Cycle × Ongoing-task demand	−0.013	0.002	−6.798	< .001				
Ongoing-task demand × Block	−0.070	0.013	−5.239	< .001				
Cycle × Block	−0.009	0.002	−5.895	< .001				
Treatment × Ongoing-task demand	−0.013	0.024	−0.568	.570				
Cycle × Treatment	0.005	0.002	1.965	.049				
Treatment × Block	−0.086	0.021	−4.190	< .001				
Treatment × Ongoing-task demand × Block	0.006	0.002	2.611	.009				
Treatment × Ongoing-task demand × Cycle	0.007	0.004	1.849	.064				
Treatment × Ongoing-task demand × Block	0.069	0.030	2.338	.019				
Treatment × Block × Cycle	0.010	0.003	3.194	.001				
Treatment × Block × Cycle × Ongoing-task demand	−0.010	0.004	−2.128	.033				

Note. In this table, *p*-values are reported only as exploratory information. Parameter estimates and tests on error rates in ongoing–task performance could not be performed due to the low number of data points.

Erklärung gemäß § 5 der Promotionsordnung

Versicherung

Hiermit versichere ich, dass ich die vorliegende Arbeit ohne unzulässige Hilfe Dritter und ohne Benutzung anderer als der angegebenen Hilfsmittel angefertigt habe. Die aus fremden Quellen direkt oder indirekt übernommenen Gedanken sind als solche kenntlich gemacht. Die Arbeit wurde bisher weder im Inland noch im Ausland in gleicher oder ähnlicher Form einer anderen Prüfungsbehörde vorgelegt.

Die vorliegende Arbeit wurde an der Professur für Allgemeine Psychologie der Technischen Universität Dresden unter wissenschaftlicher Betreuung durch Prof. Dr. Thomas Goschke (Technische Universität Dresden) angefertigt.

Bisher habe ich an keinem anderen Promotionsverfahren teilgenommen.

Ich erkenne die Promotionsordnung des Bereichs Mathematik und Naturwissenschaften der Technischen Universität Dresden vom 23.02.2011, zuletzt geändert am 23.05.2018, an.

Dresden, 15.05.2019

Marcus Möschl